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A STUDY OF SOME RELATIONS AMONG APERIODIC REINFORCEMENT, DISCRIMINATION TRAINING, AND SECONDARY REINFORCEMENT [1]

BY J. M. NOTTERMAN [2]

Columbia University

Although the phenomenon of secondary reinforcement has been variously demonstrated, systematic and parametric investigations of the operations involved have only recently appeared (e.g., **1**, **3**, **13**). A comprehensive review of the literature may be found in (**8**) and (**11**).

The present study was suggested by the fact that the procedures used in establishing a secondary reinforcing stimulus (S^r) have been the same as in the formation of a discrimination with respect to this stimulus (cf. **14**). For example, in the bar-pressing situation used by Skinner (**16**) and Bugelski (**2**), tray-approach responses following a "click" were reinforced, while responses not so preceded went unreinforced and, hence, tended to become extinguished. Thus, it seemed desirable to establish an S^r under conditions carefully controlling the emission of unreinforced responses in the absence of S^D,[3] in order to determine the relationship of secondary reinforcement to the proportion of such responses during training. The runway technique seemed to offer an adequate means of carrying out such controlled discrimination training.

STATEMENT OF PROBLEM

The central aim was to investigate secondary reinforcement as a function of the number of S^Δ trials in discrimination training. The procedure adopted also yielded data concerning: (1) discrimination as a function of this procedure, and (2) response

[1] Submitted in partial fulfillment of the requirements for the Degree of Doctor of Philosophy in the Faculty of Pure Science, Columbia University, January, 1950. The writer is at present post-doctoral Research Fellow of the National Institute of Mental Health.

[2] The writer wishes to express his appreciation to Professor W. N. Schoenfeld for his constant guidance during all phases of the study here reported. Thanks are also due Professor F. S. Keller and Mr. D. E. Page for critically reading the manuscript. Lastly, the writer is indebted to Dr. P. J. Bersh for aid and encouragement throughout the course of the research.

[3] S^D is a discriminative stimulus (e.g., "click") correlated with a reinforced response. An S^Δ trial, as used in the present experiment, refers to an unreinforced trial during which a given discriminative stimulus is absent. The notation is that of Skinner (**16**).

strength during conditioning and extinction as a function of varying schedules of aperiodic reinforcement (APR).

Apparatus

Columbia University Skinner Box.—The living-experimental cages, together with the levers used in conjunction with them, differed little from the model described in (6). A 6-w., 120-v. bulb was mounted on the perforated metal cover of the cage so that its rays were directed downward through several layers of filter paper into the cage. Illumination on the floor of the cage was 2.7 ft.-candles with this light, and 0.09 ft.-candles without it. The light was normally off, coming on for 1.00 ± .01 sec. following each bar depression.

Runway.—This was a trough-type, wooden runway with a screen top, painted flat black. The essential components of the runway can be

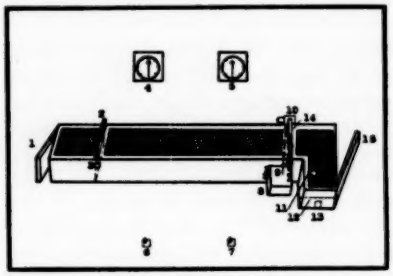

Fig. 1. Runway apparatus

located in Fig. 1 by reference to the numbers corresponding to those given in the following description. The animal was placed in the starting box through an outside door, *1*. An inside door, *2*—hereafter referred to as door *A*—hand-operated by a pulley, *3*, gave access to the runway proper. The entrance door to the goal box, *14*, also pulley-operated, was separated from door *A* by approximately 4 ft. of straight runway. Just beyond the entrance door of the goal box, the runway turned a right angle, and gave access after approximately 1.5 ft. to the food tray, *13*. A side of the goal box was hinged to provide an exit door, *15*.

A light source, *8*, and photo-relay, *10*, were located on opposite sides of the runway so that the rat would intercept the light beam, *9*, just before entering the goal box. This beam, provided by a Mazda 55 lamp, passed through a gelatin infra-red filter (88 Angstroms), and was invisible to the human eye. Evidence available indicates that it was also subliminal for the albino rat (17).

Upon interruption of the beam, a 0.01-sec. Standard Electric Timer, *4*, automatically started by the opening of door *A*, was stopped by means of a switch, *6*; and another similar timer, *5*, was simultaneously started. The latter was stopped by means of a switch, *7*, when the rat crossed a white line, *12*, on the floor of the runway 1 in. in front of the food tray, *13*. The goal-box door, *14*, was then lowered.

Interruption of the beam which was present on all trials controlled a 6-w., 120-v. bulb, *11*, used as an S^D. Whether or not the light came on upon beam interruption was determined independently by *E*. This light was mounted on the top of the goal box so that its rays were directed downward through a milk glass filter. Goal-box illumination with this light was 4.4 ft.-candles; without it, 0.14 ft.-candles. Available data indicate that a 30:1 ratio is almost maximally discriminable by the albino rat at this level of illumination (5). The duration of the light, 35.00 ± 0.03 sec., was controlled by an electronic timer.

Procedure

A total of 81 male Wistar rats, 90 to 110 days old, was used. A set of 20 (in one case, 21) rats of mixed experimental groups was worked with during each of four training cycles. Each cycle lasted 23 days, and all members of a given set were worked with during each day of a particular cycle—two animals during each of ten 45-min. experimental sessions.

There were seven phases to each cycle: (1) feeding rhythm (3 days), (2) bar-pressing operant level (6 days), (3) runway acquisition (6 days), (4) transfer test (3 days), and (5), (6), and (7) "reconditioning," extinction, and test for secondary reinforcement (5 days).

Feeding rhythm.—Rats were fed ground Purina Laboratory Chow for 45 min. daily in the living-experimental cages, which were cleaned at the end of each feeding period. The latter started 15 min. after the assigned experimental period of 45 min. This rhythm was begun three days before the first day of operant level and was maintained throughout the experiment. Water was constantly available.

Operant level determinations.[4]—The unreinforced bar-pressing activity of each rat was recorded for a 45-min. period on each of six consecutive days (see 15). A 1-sec. light followed each bar depression. This phase also served to increase to nine days the rhythm period preceding runway acquisition.

[4] Data bearing on this procedure are reported elsewhere (12).

Runway acquisition.—The 45-min. experimental sessions of the succeeding six days were devoted to acquisition training. The 81 rats were divided into one control group and five experimental groups, each of which received a total of 50 reinforced trials and varying numbers of unreinforced trials. Eight reinforced trials were given on each of the first five days; 10 on the sixth. The total number of unreinforced trials was varied from group to group as follows: Group I ($N = 14$), 0; Group II ($N = 13$), 10; Group III ($N = 14$), 25; Group IV ($N = 13$), 50; Group V ($N = 14$), 100; Control Group ($N = 13$), 25. No unreinforced trials were given the first day; one-fifth of the group total was given on each of the ensuing five days.

On all reinforced trials, (*a*) the light came on for 35 sec. as the rat entered the goal box (control group excepted), and (*b*) the rat was permitted to eat from the food tray for 30 to 40 sec. (consuming three or four 0.1 gm. Purina pellets). The tray was kept full so that the supply would never be exhausted while the animal was in the goal box in the presence of light.

On all unreinforced trials, (*a*) the light did not come on, and (*b*) the food tray was not present. The animal was kept in the goal box for 30 to 40 sec.

Approximately 40 sec. elapsed between removal of the rat from the goal box, and opening of door *A* for the next run.

Except for the control group, the order in which the assigned number of unreinforced trials was interspersed on each day was determined separately for each animal by means of Fisher's table of random numbers (4). Each control rat was given the same schedule as a paired rat in Group III. Thus, except for the omission of the light on reinforced trials, the control group procedure was identical with that of Group III.

Inasmuch as the light (S^D) was present on all reinforced trials for the experimental groups, these trials may be referred to as S^D trials. Conversely, the unreinforced trials for these groups constitute S^Δ trials.

From the foregoing, it can be seen that (*a*) the total number of reinforced trials remained constant from group to group, (*b*) the total number of unreinforced trials was the independent variable, (*c*) the reinforced and unreinforced trials were randomized, and (*d*) conditions up to the point of beam interruption were identical for all groups. Accordingly, the time elapsing from opening of the starting-box door to beam interruption may be used to determine the effect of varying schedules of aperiodic reinforcement.

Attention is called to the function of three features of the runway apparatus in reducing uncontrolled S^Δ responding to a minimum: (*a*) Door *A* prevented unscheduled approaches to the goal box; (*b*) the possibility that incipient "straight-ahead" responses in the starting box would serve as S^Δ training was reduced by the requirement of a right turn immediately before reinforcement; (*c*) the "distinctness" of the right-turn response was emphasized by the extent of the preceding straight-ahead response.

Test for transfer.[5]—Each rat spent 45 min. in the Skinner Box on each of three days following runway acquisition. Procedure was identical with that of operant level determinations as described above.

"Reconditioning." [6]—On the following day all groups were returned to the runway situation. Group I (the regularly reinforced group) was given three successive S^D trials. Groups II–V (the APR groups) were each given three S^D trials, alternated with three S^Δ trials. The control group was given three reinforced trials, alternated with three unreinforced trials. Note that the S^D and S^Δ conditions were identical with those maintained during acquisition, and that each animal received three reinforced trials.

Extinction.—The first extinction trial immediately followed the last "reconditioning" trial. Each group was divided into two subgroups matched for response strength in terms of mean log *A*-time (defined below) on the last day of acquisition. One subgroup received the light on each trial; the other did not. Each of the "dark" subgroups had an N of 7. The food tray was not present for either group. Time in goal box and between trials was as in acquisition. The number of trials given on the five extinction days were, in order, 14, 20, 20, 25, and 25, making a total of 104 trials.

Test for secondary reinforcement.—For extinction trials 94 to 104, the light conditions were reversed for all subgroups.

Results and Discussion

The results are presented in terms of two runway time measures, *A*- and *B*-time. *A*-time is the time in sec. from the opening of the starting box door *A* to interruption of the beam at the entrance to the goal box. *B*-time is the time from goal box entry to crossing of the white line in front of the food tray location. Data are presented mostly in terms of mean log *A*-time. The time measures of each

[5] Data bearing on this procedure are reported in (12).

[6] Data bearing on this procedure are reported in (12).

trial were transformed into logarithms in order to compensate for the skewed distributions characteristic of runway measures (see 7).

If either an *A*-time or a *B*-time extended to 3 min., the trial was terminated and given a time of 180 sec. In only one case was the 3-min. criterion reached on *A*-time during acquisition. This animal never finished the eight trials scheduled for the first day, and was discarded. The presence of criterion trials during other phases of the experiment is discussed in connection with the pertinent graphs.

Runway acquisition.—A family of curves for the five experimental groups, showing running time (mean log *A*-time) as a function of successive training days, is presented in Fig. 2. The number of trials determining a given point is the *N* for the group, times the number of trials given on that day. Intergroup comparisons show that running time decreases as the number of interspersed unreinforced trials increases, reinforced trials being held constant. Mean log *A*-time is approximately the same for all groups on Day 1. An alignment on the basis of the number of unreinforced trials begins on Day 2, when the independent variable is introduced, and is maintained thereafter. That the family of curves is not an artifact of total number of trials is indicated by the fact that each group appears to approach different asymptotes in a regular order. A plot of mean log *A* with "total trials" on the abscissa shows the same phenomena.

Inspection of the five values for Day 6 indicates that final running time is a decreasing monotonic function of the total number of unreinforced trials (see bottom curve in Fig. 8). In view of the nature of the independent variable, it would seem likely that this function would reverse itself at some point beyond the range

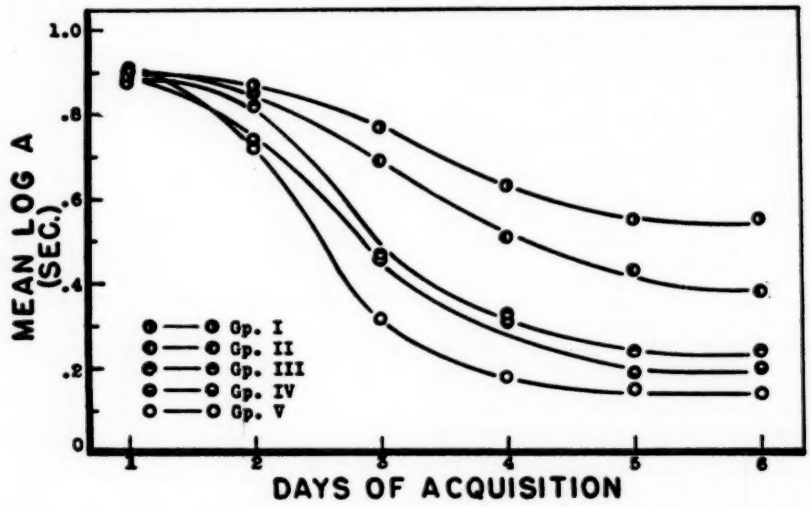

FIG. 2. The effect of varying conditions of aperiodic reinforcement during acquisition. The number of reinforced trials was held constant; the number of interspersed unreinforced trials varied from group to group.

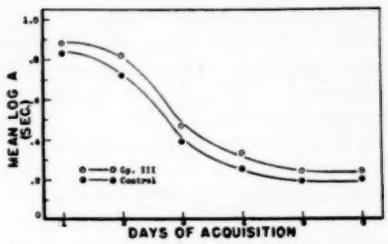

FIG. 3. Course of acquisition for Group III and Control

of data covered in the present experiment. The points for Groups I and II are significantly different at the .05 level; for Groups II and III, at the .02 level. The Group IV value does not differ significantly from that of Group III or Group V.

In a recent review and critique of various partial reinforcement experiments, Jenkins and Stanley conclude that ". . . acquisition proceeds somewhat more rapidly and reaches a higher final training level under continuous reinforcement than under partial reinforcement" (**9**, p. 209). This conclusion seems to be at variance with the acquisition data of the present experiment. However, Jenkins and Stanley qualify their conclusion by saying that (*a*) "instances can be found indicating either higher or lower response strength in the partial instance," (*b*) their generalizations have particular application for experiments involving small frequencies of reinforcement, and (*c*) "random ratio reinforcement schedules need more investigations." The present experiment is an example of a "random reinforcement" schedule.

The acquisition curve for the control group (Fig. 3) closely parallels that of Group III, being slightly lower at all points. Since the procedures for the two groups differed only in that the control group did not receive the light on reinforced trials, the slight separation of the curves would seem amenable to one of two interpretations: (*a*) the two groups of rats did not come from the same parent population, or (*b*) the light accompanying reinforcement in Group III slightly retarded the running response. On the first trial of acquisition (prior to the first presentation of light to Group III), Group III and the control group have quite similar log *A*-times—.9583 and .9622, respectively. While this conformity does not completely rule out the possibility of sampling differences, explanation in terms of light aversion receives support from many investigators (**10, 16, 18**). In so far as the difference may be attributable to the light, it may be surmised that the curves of Fig. 2 are higher on the ordinate than would be the case if the light had not been introduced.

The effect of the training procedure on final (Day 6) *B*-time values for experimental groups is presented in Fig. 4. The *median* log *B*-time values for S^{Δ} trials increases as a function of the total number of S^{Δ} trials.[7] The

[7] The median is used here instead of the mean because of the presence of a comparatively large number of S^{Δ} criterion runs. When the discrimination had become well established, some animals occasionally would not continue into the goal box after running down to the point of beam interception and not receiving the light. Al-

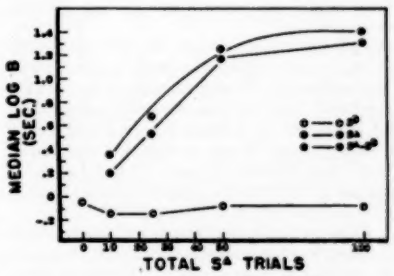

FIG. 4. S^{D} and S^{Δ} strength on last day of acquisition, as a function of total number of S^{Δ} trials, S^{D} trials being held constant

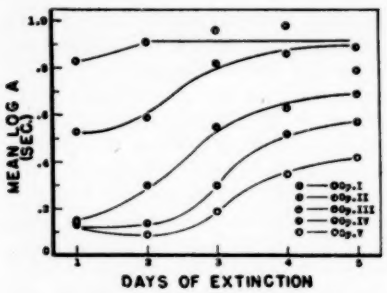

FIG. 5. The effect of varying conditions of aperiodic reinforcement upon resistance to extinction

same function of median log B for S^D trials shows little difference from group to group.[8] In terms of *mean* log B-time, the only significant intergroup difference for S^D trials is that between Group III and IV, which is significant beyond the .01 level.

In the uppermost curve in Fig. 4, each point represents the difference between the median log B of S^Δ and S^D trials, respectively. In terms of this differential, it may be said that the effectiveness of the discrimination increases as the number of S^Δ trials increases (see **5**). The median log B for reinforced and unreinforced trials of the control group were, respectively, $-.189$ and $-.199$.

Extinction.—Comparison of mean log A-times for each of the five extinction sessions revealed no significant subgroup differences within a given group (**12**). Therefore, data for the two subgroups of each group were combined, yielding the family of curves presented in Fig. 5. Mean log A is based on the first five trials for a given day, since later trials included criterion trials (runs terminated at 180 sec.). The frequency of these criterion trials does not vary systematically from group to group. The five-trial means of Fig. 5 contain criterion runs only for Group II, one animal of which gave two criterion runs on each of the last three days. (Despite the influence of criterion runs on the means, extinction curves based on 10-, 14-, and total-trial means, while less regular, show the same general trends as Fig. 5.)

A comparison of the resistance to extinction of the experimental groups is afforded by determining the first extinction session during which a given group's mean log A reaches and maintains a level significantly (.01) higher than its mean log A on the last day of acquisition. The t-tests reveal the following: Group I is significantly higher on the first day, Group II on the second day, Group III on the third day, Group IV on the fourth day, and Group V on the fourth day. After once reaching this significance level for extinction, none of the groups falls below on succeeding extinction sessions.

Test for secondary reinforcement.—Although it is apparent from the foregoing that the experimental groups proceed through extinction differently, examination of later extinction trials on Day 5 indicates that some degree of intergroup equality in running time is attained. This is not inconsistent with the argument advanced in the preceding section, because it is quite possible that the means based only on the first five

though the median is used, the S^Δ curve based on the means shows the same general trend. The number of S^Δ trials in each group, and the number of criterion runs occurring among these S^Δ trials, was as follows: Group I had no S^Δ training; Group II had no criterion runs out of 26 S^Δ trials on Day 6; Group III had 1 criterion run out of 70 S^Δ trials; Group IV had 17 out of 130; and Group V had 18 out of 280.

[8] The median is used here in order to facilitate a comparison between the S^D and the S^Δ curves. No criterion S^D runs occurred. The shape of the curve shows the same trend whether median or mean is used. The number of trials represented by each point is the group $N \times 10$.

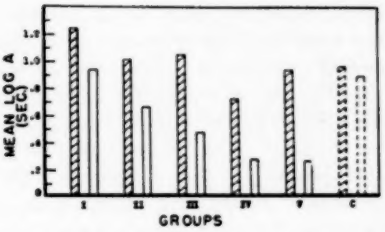

FIG. 6. Test for secondary reinforcement. The cross-hatched columns are the mean log *A*-times of the last five extinction trials in the dark. The blank columns are the mean log *A*-times of the first five trials after reinstatement of the light. The broken columns on the extreme right are the Control data.

trials of a given session are more sensitive to intergroup differences than the means based only on later trials.

Specifically, for the six subgroups which did not receive the light during extinction (the "dark" subgroups), only 1 of the 15 possible intergroup comparisons of mean log *A* is significant on the last five trials immediately preceding reinstatement of the light (trials 90–94). This one significant difference (.01 level), between Groups I and IV, does not reflect any systematic group-to-group trend (see Fig. 6).

For each of these subgroups, however, mean log *A* for the first five trials after light is reinstated is systematically less than that for the preceding five trials. The decrease is not significant for the control group and Group I; for Group II, it is significant between .05 and .02; for Groups III, IV, and V, beyond .01.[9]

[9] Seven criterion trials are represented among the experimental "dark" subgroup data, before the light was introduced. They were distributed as follows: 0 in Group I, 4 in Group II, 1 in Group III, 0 in Group IV, and 2 in Group V. The presence of only seven criterion runs among the 175 trials (35 trials for each of five experimental subgroups) does not materially alter the central tendency. Moreover, if the three animals concerned had been allowed to remain in the runway beyond the 3-min. criterion, the increased *A*-time of "before change" trials would have accentuated the differences between the before and after means. There were no criterion runs in the "after" trials.

Differences in mean log *A*-time between the five trials before and after the change are not significant for any of the six "light" subgroups. Of the experimental "light" subgroups, two increase and three decrease slightly in running time when the "dark" condition is instituted. The control subgroup gives a small decrease, comparable to that of the other control subgroup.

The differential effect upon the five experimental "dark" subgroups of the reinstatement of the light condition is shown in Fig. 7 in terms of *median* log *A* (bottom curve), and in Fig. 8 in terms of *mean* log *A* (top curve). Taking running time as an inverse measure of response strength, these curves indicate that the secondary reinforcing effect is an increasing monotonic function of the total number of S^{Δ} trials in the discrimination training procedure.

Figure 7 also shows that this change of conditions results in an alteration of log *B* comparable to that obtaining

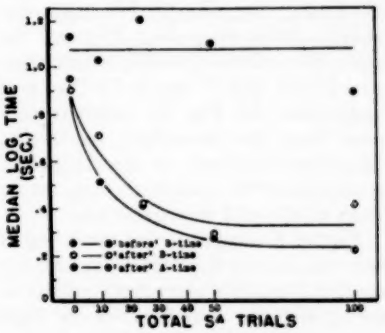

FIG. 7. Effect of dark-to-light change upon *B*-time. *A*-time after change to light is plotted for comparison purposes.

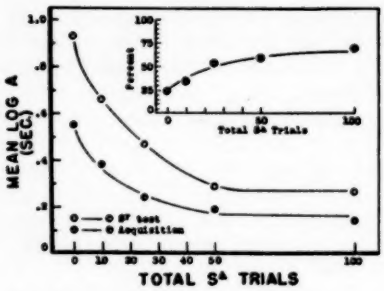

FIG. 8. Extent of recovery of response strength as measured by *A*-time. Mean log *A* values of dark subgroups after change to light, compared to mean log *A* values on last day of acquisition; both taken as a function of total S^{Δ} trials during acquisition. Inset is explained in text.

for log *A*. For the five trials before the change (89–93), median log *B* does not differ systematically among these experimental subgroups. For the following five trials, however, the median log *B* function is quite similar in form to the corresponding median log *A* function. This is consistent with the recent finding of Dinsmoor (**3**) that, under comparable test conditions following discrimination training, the secondary reinforcing function and the discriminative function of a stimulus seem to be quantitatively identical. However, the sharp difference between the median log *B* curve for these five trials (94–98), when compared with the S^{D} curve for Day 6 of acquisition (cf. Fig. 4), seems to suggest that the secondary reinforcing function is related to discriminative strength at the time of testing, rather than at the end of acquisition.

Figure 8 compares the mean log *A* function for the five "dark" subgroups on the five trials after light reinstatement, with the same function for these groups on the last day of acquisition. A comparison of the two curves shows that the differential effect of light reinstatement is such as to establish the same ranking of groups as at the end of acquisition. The inset shows that the per cent decrease in running time differs systematically from group to group.[10] The percentage curve also points to the conclusion that secondary reinforcing strength is a function of discrimination training in which the independent variable is the number of S^{Δ} trials.[11]

Summary and Conclusions

A total of 81 Wistar rats, consisting of five experimental groups and one control group, were trained on a trough-type runway. The experimental groups received a constant number of reinforced light-present (S^{D}) trials, but differing numbers of unreinforced light-absent (S^{Δ}) trials.

The following main conclusions, which are based only upon the range of data covered in this experiment, are drawn:

(1) The acquisition by a stimulus of secondary reinforcing power is an increasing monotonic function of the number of S^{Δ} trials given during a previous discrimination procedure, S^{D} trials being held constant.

(2) The effectiveness of a discrimination, as measured by the difference in strength between S^{D} and S^{Δ}, is a

[10] For each subgroup, the percentage was calculated by subtracting the first five-trial mean log *A*-time after change from the last five-trial mean log *A*-time before change, and dividing by the latter.

[11] Since over 15,000 trials were given, it did not prove practical to include the raw data in this article. The trial-by-trial and the daily mean data for each rat are on file with the Department of Psychology, Columbia University. The daily mean data may also be secured from the American Documentation Institute, 1719 N Street, N.W., Washington 6, D.C., by ordering Document 3229, remitting $1.00 for 35 mm. microfilm or $2.40 for 6 × 8 in. photocopies.

function of the number of S^{Δ} trials, when S^{D} trials are held constant.

(3) The strength of an aperiodically reinforced response, as measured by running time during acquisition, is an increasing monotonic function of the number of randomly interspersed unreinforced trials, reinforced trials being held constant.

(4) The resistance to extinction of an aperiodically reinforced response, as measured by running time, increases as a function of the number of randomly interspersed unreinforced trials received during acquisition, when the number of reinforced trials is held constant.

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THE EFFECT OF PRACTICE ON THE PERCEPTION OF OBSTACLES BY THE BLIND

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In a previous study (**8**), the totally blind students at the Texas State School for the Blind were given 60 trials in an obstacle test course to determine their ability to perceive obstacles. Of the 34 *Ss*, 27 or 79 per cent detected the obstacle on almost every trial in which the obstacle was presented and rarely reported its presence in the "check" or "blank" trials. This high percentage was contrary to the impressions of other investigators (**1**) who felt that an adequate testing of the blind population would reveal a fairly small percentage capable of utilizing the sensory cues available in a test situation. It is true that in evaluating the ability to perceive obstacles, we must consider the kind, size, shape, and position of the obstacles. It was a belief, however, in the general inability of the blind to perceive obstacles that led to recent research in obstacle-avoidance devices. A number of devices using supersonic, sonic, and light beams have already been developed and tested (**1, 3**). An evaluation of the ability of the blind person to orient himself without a device is necessary before the efficiency of the device can be determined.

The present investigation is designed to determine whether the seven *Ss* who failed in the previous experiment by reporting two or more "false" perceptions on the "check" trials, or by colliding with the obstacle six or more times, or by giving mean "final appraisals" which increased with the distance of the starting position, could, under conditions favorable for learning, develop this ability. The fact that this performance could be learned has been well demonstrated in other investigations. Supa, Cotzin, and Dallenbach (**4**), in their experiment on the essential cues in obstacle perception, found that their two naive sighted *Ss* reached the criterion of 25 successes (that is, trials in which the *S* did not collide with the obstacle) in 44 trials. Worchel and Ammons (**5**), using 10 sighted *Ss*, studied the course of learning in the perception of obstacles, and found that their *Ss* required 30 to 90 trials to perceive the obstacle 25 times in 30 trials. The Cornell experiments (**2, 4, 7**) have definitely shown that the essential sensory requirement for the successful perception of obstacles is hearing. Deafness for tones above 8000 cycles (**2**) prevents the acquisition of this ability (**6, 7**). Worchel and Dallenbach (**7**) set up a training series of 300 trials for 10 deaf-blind *Ss*. At the end of that time, however, their *Ss* showed no improvement.

It might easily be argued that any training program in obstacle perception for *Ss* who have been blind for some length of time is futile since they already have been exposed to considerable practice. If they could have developed this ability, they would have done so early in their blindness. However, Jerome and Proshansky (**3**), in comparing the efficiency of blind *Ss* to perceive obstacles with and without a device, found that "the surprising thing about these results is the dramatic improvement in the performance of sophisticated blind subjects as a result of training in the detection of obstacles. To be sure, this might

apply only to the specific situation which obtains in an obstacle course. There is, however, the possibility that systematic training in obstacle detection could greatly improve the performance of many blinded individuals, even including those who already have some skill. A further study of these possibilities, and of the training techniques required, would seem desirable."

Procedure

The seven totally blind *Ss* who failed in the previous experiment (8) in the obstacle test course participated in the present experiment. These *Ss* reported two or more "false" perceptions in the six "check" trials, or collided with the obstacle more than five times in the 54 trials with the obstacle. Their mean "final appraisals" increased with the distance of the starting position from the obstacle. Table I presents the sex, age, duration of blindness, school grades, and the report from the oculist on these *Ss*. Audiograms were all normal.

The training and testing took place outdoors on a concrete walk 65 ft. long and 8 ft. wide. This walk bisected a grass-covered plaza which was bounded on all sides by streets and buildings. The nearest building was approximately 100 ft. distant.

In all trials the obstacle which was placed perpendicular to the path was an unpainted masonite board 4 ft. wide and 4 ft. 10 in. high. This board was attached to a wooden frame mounted on ballbearing rollers, so that its lower edge was 2 ft. above the walk and its upper edge was 6 ft. 10 in. above the walk.

The blindfold used was a pair of blackened U.S. Navy Variable Density Goggles. These goggles were a most excellent blindfold because they fitted snugly to the face, and because of the knob that protruded from them. In cases of collision, this knob served as a buffer to catch the shock and thus protect the more vulnerable portions of the face.

The experiment was conducted in the afternoons. The training session consisted of giving each *S* seven series of 30 trials each, or a total of 210 trials in the obstacle course. Two series with a 5-min. rest interval between were given daily. On the fourth day, only one series was given.

Each day, prior to the first trial, the following instructions were read to the *S*: "A blindfold will be placed over your eyes so that you will not have more skin showing than sighted people who have done this experiment previously. After you are blindfolded, you will be led about this area, in order that you will not know how far you are away from this movable obstacle. After a short time, you will be placed facing the obstacle near the right-hand side of the sidewalk. When you are tapped on the back, walk forward toward the obstacle. Walk near the right edge of the sidewalk and use this edge as a guide toward the obstacle. If you should leave the walk, return to the walk at once and continue on toward the obstacle. When you first feel that something is in front of you, stop and raise your right arm. Then, lower your arm and continue to walk toward the obstacle. Approach it as closely as possible without touching it. When you have done that, raise your left arm and stand still. As we wish to know how well you can do this, you will be asked to approach the obstacle many times."

After the instructions were read to the *S*, a blindfold was placed over his eyes. The *S* was then led about the experimental area to disorient him, and then placed haphazardly 6, 12, 18, 24, 30, or 36 ft. from the board.

The position of the obstacle and of the starting position were both varied. This insured that the *S* could not possibly ascertain the position of the obstacle through the medium of sec-

TABLE I

A Description of the Seven *Ss* in the Present Study

S	Sex	Age	Age at onset of blindness	Present School Grade	Eye Diagnosis
JB	F	16	4	11	Calcarious Lens
JD	M	20	5	11	Optic Atrophy
RH	M	10	3	5	Anophthalmos, Uveitis
JM	M	16	5	11	Juvenile Cataract
DP	F	16	Birth	11	Optic Atrophy
BW	M	18	11	9	Surgical Anophthalmos
GK	M	17	5	11	Atrophy Bulbi Phthisis Bulbi

ondary cues derived from the irregularities of the sidewalk.

The *S* gave two reports: When he first perceived the obstacle ("first perception") and when he thought he was as close as he could get without touching the obstacle ("final appraisal"). The first perceptions were measured to the nearest ft. and the final appraisals to the nearest in. Before each trial, the *S* was led by the *E* in a circuitous route on the grass to disorient him.

The *Ss* always approached the obstacle from the west in order to prevent the shadow of the obstacle from falling on the *S* as he neared the board.

In order to facilitate learning as much as possible, the following rules of procedure were utilized. (1) *Punishment:* The *S* was not prevented from colliding with the obstacle. (2) *Reward:* If *S*'s appraisals were 1 ft. or less from the obstacle, he was praised by a pat on the back and told that his performance was good. (3) *Withholding reward:* Whenever appraisals were more than 1 ft. or the performance was much poorer than previous ones, praise was omitted. (4) *Knowledge of results:* After every appraisal, *S* was led up to the obstacle so that he could know the amount of his error.

The test session was given after each *S* had completed his 210 training trials. The procedure of the test trials was the same as that used in the previous experiment (8). Sixty trials were given. After each *S* had given his "final appraisal," however, he was not led up to the board. No comments were made at any time during this series. Six "blank" or "check" trials, in which the obstacle was placed not in *S*'s path but flat on the ground to one side, were introduced at random as in the previous experiment. None of the *Ss* knew that these "blank" trials were to be introduced. On these "blank" trials, if the *Ss* did not give any report during the entire length of the walk (65 ft.), the *E* would merely start him on the next trial without making any comment.

Results

In order to show the effect of the training session on the ability of our blind *Ss* to perceive obstacles, we have presented in Table II the results for each *S* of the 60 pre-practice trials of the previous experiment (8). Table III shows the results of the 60 post-practice trials given under similar conditions in the present experiment. The improvement in the ability of all the blind *Ss* to perceive the obstacle is evident.

In the pre-practice session, the means of the "first perceptions" and "final appraisals" increase for all the *Ss* with the distance of the starting points, i.e., the farther the starting distance the larger the means. For

TABLE II

The Mean Distance and Variation (in ft.) of the "First Perception" and "Final Appraisal" for Every Starting Position, the Total Number of Collisions and "False Perceptions" in the Pre-Practice Series (60 Trials) *

S	Report	Starting Distances from Wall (in ft.)						Total Collisions	False Perceptions
		6	12	18	24	30	36		
JB	First Perception	1.36 ±0.69	2.94 ±0.95	5.50 ±2.63	7.78 ±4.20	8.67 ±6.89	9.67 ±5.19	5	6
	Final Appraisal	0.65 ±0.45	1.23 ±0.68	1.03 ±0.28	2.22 ±1.07	1.25 ±0.33	2.11 ±1.14		
JD	First Perception	2.72 ±1.20	4.11 ±1.46	6.38 ±1.47	6.44 ±1.38	9.11 ±3.26	12.33 ±5.11	16	6
	Final Appraisal	1.08 ±0.68	1.88 ±0.63	1.36 ±0.55	1.88 ±0.44	3.40 ±1.70	3.14 ±1.31		
RH	First Perception	0.99 ±1.01	0.89 ±0.42	0.86 ±0.51	0.58 ±0.36	1.36 ±1.29	4.53 ±5.76	16	3
	Final Appraisal	0.28 ±0.24	0.18 ±0.08	0.12 ±0.03	0.13 ±0.04	0.39 ±0.16	0.24 ±0.09		
JM	First Perception	6.00 ±0.00	9.78 ±2.96	12.22 ±5.14	14.88 ±6.82	14.55 ±8.84	23.62 ±6.07	7	2
	Final Appraisal	0.39 ±0.31	1.09 ±0.60	1.66 ±1.26	1.07 ±0.93	1.07 ±0.68	2.89 ±3.14		
DP	First Perception	5.67 ±0.33	7.80 ±2.20	8.89 ±2.10	7.44 ±2.59	8.44 ±4.69	8.44 ±3.04	0	3
	Final Appraisal	1.56 ±0.62	2.50 ±1.67	4.17 ±2.52	3.56 ±3.41	4.17 ±4.59	5.56 ±3.95		
BW	First Perception	5.89 ±1.97	9.33 ±1.93	12.44 ±2.72	17.44 ±4.84	23.44 ±2.52	23.66 ±7.26	3	6
	Final Appraisal	0.96 ±0.42	1.25 ±0.44	1.44 ±0.60	1.21 ±0.47	3.39 ±2.80	1.00 ±0.29		
GK	First Perception	6.00 ±0.00	10.00 ±1.11	13.66 ±3.04	19.33 ±2.89	26.66 ±2.00	28.22 ±4.37	9	5
	Final Appraisal	0.70 ±0.34	0.63 ±0.37	0.58 ±0.35	0.41 ±0.30	2.19 ±2.81	0.80 ±0.48		

* From Worchel, P., Mauney, J., and Andrew, J. (8).

TABLE III

THE MEAN DISTANCE AND VARIATION (IN FT.) OF THE "FIRST PERCEPTION" AND "FINAL APPRAISAL" FOR EVERY STARTING POSITION, THE TOTAL NUMBER OF COLLISIONS AND "FALSE PERCEPTIONS" IN THE POST-PRACTICE SERIES (60 TRIALS)

S	Report	Starting Distances from Wall (in ft.)						Total Collisions	False Perceptions
		6	12	18	24	30	36		
JB	First Perception	3.30±1.00	3.70±1.30	3.90±1.20	2.70±1.60	5.00±2.20	6.10±2.30		
	Final Appraisal	0.58±0.30	0.66±0.19	0.69±0.42	0.42±0.30	0.72±0.28	0.94±0.47	1	1
JD	First Perception	5.30±7.40	8.00±2.40	7.40±3.20	7.30±1.80	7.60±1.70	9.20±3.40		
	Final Appraisal	0.22±0.04	0.41±0.33	0.36±0.33	0.55±0.28	0.25±0.11	0.83±0.41	5	0
RH	First Perception	5.60±0.60	6.40±4.70	4.30±1.70	3.10±0.80	4.20±2.30	5.90±3.40		
	Final Appraisal	0.33±0.24	0.36±0.33	0.30±0.19	0.22±0.13	0.30±0.17	0.19±0.13	5	0
JM	First Perception	6.00±0.00	7.50±3.50	10.00±5.33	7.00±4.50	7.50±2.30	5.50±1.30		
	Final Appraisal	0.11±0.05	0.17±0.08	0.11±0.05	0.11±0.08	0.26±0.26	0.26±0.17	1	0
DP	First Perception	4.30±1.50	6.00±0.80	7.20±1.60	7.10±1.70	9.30±1.50	6.80±2.00		
	Final Appraisal	0.22±0.13	0.22±0.11	0.28±0.11	0.33±0.13	0.47±0.22	0.36±0.19	1	0
BW	First Perception	2.50±0.60	2.60±0.60	2.50±0.90	2.50±0.60	2.30±0.60	2.50±0.80		
	Final Appraisal	0.63±0.30	0.75±0.55	0.66±0.42	0.55±0.47	0.69±0.49	0.41±0.17	5	0
GK	First Perception	5.50±0.70	6.20±0.80	5.72±1.70	5.95±2.11	7.15±2.44	6.55±2.50		
	Final Appraisal	0.33±0.13	0.28±0.11	0.45±0.17	0.36±0.15	0.47±0.11	0.26±0.17	1	0

example, JM's mean "first perceptions" at the different starting points are 6.00 at 6 ft., 9.78 at 12 ft., 12.22 at 18 ft., 14.88 at 24 ft., 14.55 at 30 ft., and 23.62 at 36 ft. His mean "final appraisals" are 0.39 at 6 ft., 1.09 at 12 ft., 1.66 at 18 ft., 1.07 at 24 ft., 1.07 at 30 ft., and 2.89 at 36 ft. These results indicated that the determining factor for the perception of these *S*s was not their perception of the obstacle but the distance of the starting points from the wall. Sheer chance within the limitations set by the distances of the starting points accounts for these results.

When, for example, the *S* started from the 6-ft. position, he could stop for his "first perception" and "final appraisal" only at points less than 6 ft. and greater than 0 ft. (the trials at which he collided with the wall—the 0-ft. points—were omitted from the calculations of the means). His means had to fall within those limits. When he started from the 12-ft. position, he could stop anywhere between the 12-ft. and 0-ft.; and similarly with the other starting points. By chance alone, the means of the trials from the different starting points would increase with the distance of the starting points. It is unreasonable to assume that the mean "final appraisals," that is, the point at which the *S* got as close to the obstacle as possible without touching it, would increase with the starting position if the *S* possessed the ability to perceive obstacles.

In the post-practice trials, greater consistency in mean "first perceptions" and "final appraisals" for each of the starting points is found. For JM, the mean "first perceptions" at the respective starting points are 6.00 ft., 7.50 ft., 10.00 ft., 7.00 ft., 7.50 ft., and 5.50 ft.; for the mean "final appraisals," the results are 0.11 ft., 0.17 ft., 0.11 ft., 0.11 ft., and 0.26 ft.

The training session not only resulted in consistent performance but in more accurate localization of the obstacle. In the post-practice trials, every mean "final appraisal" was less than 1 ft., and all but one mean

variation was less than 0.50 ft. In the pre-practice trials, the mean "final appraisals" range all the way up to 5.56 ft., with mean variations up to 4.59 ft.

A still more accurate index of obstacle perception is the number of collisions. The training session resulted in a significant decrease in collisions. Prior to training, the total number of collisions for the seven *S*s was 56. After training, the total number of collisions was only 19. The character of the collisions differed considerably. Most of the collisions in the pre-practice series were forceful and unexpected; in the post-practice series, the *S*s collided with the obstacle while "inching" up to it. They reported "knowing" the obstacle was there, but were curious as to how far they were from it. They also wanted to better their "final appraisals."

One of the best tests of the "obstacle sense" is in the number of false perceptions. The check trials in which the obstacle is removed from the path gives us an opportunity to test the chance hypothesis. Also since the *S*s did not know when the check trials occurred, or even that they were to be given, their reports in these trials may be samples of their reports in the entire experiment. Prior to training, three *S*s reported "first perceptions" in all six check trials. The mean number of false perceptions for the seven *S*s was 4.5. After training, only one *S* reported one false perception. This striking reduction in the number of false reports is all the more remarkable since the check trials were not given during the training series and were introduced in the test series without *S*'s knowledge. All the *S*s were able in the check trials to discriminate instantly the many conflicting and false cues such as shifting winds, sudden changes in sound intensity, and changes in light and temperature as

TABLE IV

RESPONSES OF BLIND SUBJECTS TO QUESTIONNAIRE ON OBSTACLE AVOIDANCE

Question	Responses by *S*						
	JB	JD	RH	JM	DP	BW	GK
1. In new places, do you collide frequently?	Not too often	Am very cautious	Yes	Yes	Would be very careful	Yes	Depends on breeze, if its not too strong
2. Have you ever used a cane?	No	No	No	No	No	Very little	Very little
3. Do parents help you get around?	Only when I go to a strange place	Yes, quite a bit	Yes	Yes, when I am with them	Only in new places	Very little	No, but father explains new layout
4. Does brother or sister help you?	A little	Yes, when I am around them	Yes	Live with grandmother, she helps	If necessary	Very little	Sister helps
5. At the school here, do you learn your way around?	I just get around	Like a reflex	Learn my way around	Go by instinct, just know where to go	Know where I am, like at home	Just learn	Use noises
6. Do you think our training has helped you avoid obstacles?	Yes	Yes, slightly	Yes	Yes, gave me an idea how close I can get to something	It has made me sense things when I am approaching	No	Yes

the sun moved from or behind the clouds.

The course of learning in the 210 training trials is very similar to that secured in a previous study by Worchel and Ammons (5). All the *Ss* showed striking improvement within the first 30 to 60 training trials. The additional trials may not have been necessary. With additional practice, performance time decreased. In some cases, the few collisions that occurred resulted toward the end of the training series when the *Ss* became more confident and began to "inch" up to the board.

In order to determine the possible factors that have prevented these blind *Ss* from acquiring the ability to perceive obstacles during their many years of blindness and to determine the extent, if any, of transfer of training from the present experiment, the *Ss* were questioned on their past and present performance. Table IV presents the questions and the responses of the individual *Ss*. It is possible that dependence on parents, siblings, or cane retarded the development of this perceptual ability. All the *Ss* reported some kind of assistance in strange places. Even under the familiar school environment when getting about is almost a matter of "reflex," all the *Ss* but one (BW) reported some kind of positive transfer.

Discussion

The present experiment shows the striking improvement in the ability of our blind *Ss* to perceive an obstacle as a result of a short training session. These results together with those of Jerome and Proshansky (3) on the improvement of sophisticated *Ss* suggest the advisability of systematic training on the blind in obstacle orientation. The trial-and-error procedures to which the blind resort in detecting obstacles are not always efficient and may often prevent or retard the development of obstacle perception. Training with all kinds of obstacles and under all sorts of conditions may yield even better results. Even under our limited conditions, transfer was evident to the extent that it made most of the *Ss* aware of the proximity of objects. A control group without any training would have been desirable if the *Ss* had been available.

The results of this experiment and of the preceding one (8) also indicate the limited use to which any obstacle advoidance device should be put. The device should not replace, but, rather, should supplement whatever ability the *S* possesses in orienting himself. It is possible that the device will be of most use in the detection of obstacles that cannot be perceived by normal cues. Low objects, very narrow objects, step-downs, etc., are handicaps which may best be perceived by some kind of obstacle device. Also, situations in which there is considerable noise may interfere with the ability of the blind person to receive specific auditory cues in detecting obstacles. A device using tactual signals may be of considerable aid in these situations. Sole dependence on the device, however, may well be a detriment. Further experimentation is necessary before the role and kind of obstacle avoidance device are determined, but this determination should depend upon the results of intensive training and exploration of the capacities of the blind *Ss* to get around their environment without a device.

The results of our questionnaire point to the possibility that dependence on parents, siblings, or cane may interfere with the acquisition of the

ability to perceive obstacles. The same questions should be presented to other experienced blind *Ss* who possess this ability before definite conclusions can be reached. The role of any such aids should be evaluated in any training program.

Summary and Conclusions

In order to determine the effect of training in the perception of obstacles, seven totally blind *Ss*, who had failed previously in an obstacle test course, were given 210 training trials under conditions favorable for learning. The post-practice test series showed for all the *Ss*:

1. Greater consistency in "first perceptions," regardless of the distance of the starting point from the obstacle.
2. Smaller and more consistent "final appraisals."
3. Fewer collisions with the obstacle.
4. Only one "false perception" in the blank trials.

Under the conditions of our obstacle course, systematic training in the perception of obstacles resulted in the development of this ability to an extent equal to that possessed by experienced *Ss*. A systematic course in perceptual recognition and detection of objects for the blind may be of considerable aid and shorten the period of trial-and-error procedure usually adopted by the blind.

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THE GROWTH AND DECAY OF REACTIVE INHIBITION AS MEASURED BY ALTERNATION BEHAVIOR

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University of Connecticut[1]

A rat's tendency not to repeat his recent responses is a very stable phenomenon. In a two-choice situation this phenomenon emerges as an "alternation" tendency. It appears under a wide variety of experimental conditions and seems to be both qualitatively and quantitatively reproducible. If a rat is run repeatedly on a single T-maze, for example, with equal reward on both goal arms, he will choose first one arm and then the other and continue to alternate between the two paths with a percentage of alternations determined by, among other things, the time between trials and the effort expended on the two arms. A detailed review of the literature relevant to the alternation phenomenon has been published recently by Solomon (**10**), who at the same time has made a theoretical contribution in this area by pointing out the applicability of Hull's construct, reactive inhibition, in an explanation of a rat's tendency to avoid repeating his last response. A simple restatement of Hull's Postulate 8 helps to make clear the relationship between theory and alternation data:

Postulate 8. Whenever a reaction (R) is evoked in an organism there is created as a result a primary negative drive (D); (a) this has an innate capacity (I_R) to inhibit the reaction potentiality (${}_sE_R$) to that response; (b) the amount of net inhibition (I_R) generated by a sequence of reaction evocations is a simple linear increasing function of the number of evocations (n); and (c) it is a positively accelerated increasing function of the work (W) involved in the execution of the response; (d) reactive inhibition (I_R) spontaneously dissipates as a simple negative function of time (t''') (**6**, p. 300).

Applying this postulate to the single T-maze situation frequently used to study the alternation tendency, the reaction evocation potential (${}_sE_R$) associated with both goal arms would be approximately equal as a result of the equal reward of both responses, but the inhibitory potential (I_R) would be strongest for the response made most recently in time, since the I_R generated by that response would not have had as much time to dissipate spontaneously. Given the other relations between excitation, inhibition, and performance in Hull's theory, stronger inhibition of the most recent response in time yields a prediction of an alternation tendency.

Alternation behavior may therefore be explained with the aid of the same construct (I_R) as has been used in giving an account of such other behavioral phenomena as spontaneous recovery, the superiority of spaced over massed practice, reminiscence, extinction, the law of least effort, and the greater ease of learning a single alternation maze with massed rather than spaced trials.

In addition to predicting the raw occurrence of the alternation tendency Hull's theory may be deductively elaborated to predict some of the determinants of the tendency. If it is accepted from the foregoing analysis that in the simple two-choice equal-

[1] The experimentation for this study was performed in the Psychological Laboratories of Brown University.

reward situation, alternation tendency is directly related to the amount of I_R built up, then alternation tendency will have the same quantitative determinants as I_R. There follow, then, from Postulate 8 three deductions: Alternation tendency will be (1) a negatively accelerated decreasing function of the time between responses; (2) a positively accelerated increasing function of the work involved in the execution of the response; and (3) a simple linear increasing function of the number of evocations.

Evidence already exists which will serve as tests for the first two deductions. Heathers (5) has shown that the percentage of alternation decreases linearly as the time interval between responses is increased to 120 sec. Beyond this interval alternation gradually approached the chance level of 50 per cent, causing the entire function to flatten out and be over all negatively accelerated as predicted by theory. Weitz and Wakeman (11) also found alternation tendency to decrease with the interval between responses up to approximately 60 sec., after which their function began to rise. Their investigation, although employing many fewer animals than that of Heathers', throws some doubt on the latter's measurement at the 120-sec. interval. Dennis (1), in reviewing the many alternation studies from his laboratory, concluded that the phenomenon is consistently reproducible with intervals of 60 sec. or less. Also, Solomon (9) has been able to reproduce Heathers' 60-sec. interval measurement of 75 per cent alternation almost exactly, so it is likely that the early part of the function at least can be considered as well established. Some confirmation of the first deduction can therefore be stated to exist.

The second deduction cited above has been investigated by Solomon (9) as an explicit test of Hull's theory He reasoned that if, indeed, I_R is responsible for alternation behavior, the tendency should be stronger when more work is involved in the reactions. This was tested experimentally by inclining the two goal arms of a single T-maze at a 16° angle from horizontal, so that greater effort would be expended by the rats in climbing to the food boxes. The added effort significantly increased the alternation percentage from 74 to 90. Further increasing the effort by loading the rats with a lead-weight saddle did not further increase this high percentage of alternation. It was pointed out correctly by Solomon that Hull's theory was the only one extant which would predict both temporal and effort effects.

The third deduction from Hull's theory has not as yet been subjected to empirical test. It is one of the purposes of the present paper to do this. If the amount of reactive inhibition is built up by increasing the number of identical responses in a short period of time (i.e., massing of trials), the theory would predict that the percentage alternation would increase proportionately. A translation of this statement into experimental terms appropriate to the test of alternation behavior would state that on a single T-maze, other factors constant, the greater the number of times a response is forced in rapid succession to one side, the greater will be the tendency for the alternate response to occur when the opportunity is given.[2] The most im-

[2] In anthropomorphic terms this behavior might be viewed as "obstinacy" equivalent to the verbal response, "I won't go to the side I've been forced to go to"; or as "supersitious optimism" equivalent to, "Maybe there's something better on the other side this time."

portant factor which must be constant is simply that no great difference in habit exist for running to either side, a condition which is met by previously rewarding both responses a large number of times. The growth of inhibition will be demonstrated by the rise in the function relating the number of massed, forced trials to percentage alternation.

It is the added purpose of this investigation to show the decay of inhibition by the fall in the curve relating the percentage alternation to the time interval between the massed, forced trials and the test of alternation. This relationship, it is believed, may have an important bearing on the nature of I_R.

Ss and Apparatus

Animals.—The animals used in this experiment were 20 female albino rats of Wistar stock, from the Brown University rat colony. They ranged in age from 110 to 160 days.

Apparatus.—A single T-maze made of three wooden blocks, each 30 in. long, 12 in. high, and 1¾ in. wide, comprised the main piece of apparatus. At the end of each maze arm there was a feeding platform with a small cup of wet Purina Laboratory Chow mash. The maze was always situated in the same place in a well-lighted experimental room providing distinctive cues on each side of the choice point. Forced trials were given by removing one or the other arm of the T. This eliminated the single choice point and converted the maze to an L-shaped runway.

In duplicate apparatuses, each *E* collected half the data, using half the animals under all conditions. The data from the two *E*'s were not significantly different.

Procedure

Pre-training.—All animals were pre-trained by placing them on a 24-hr. feeding schedule and giving them 10 food-rewarded runs on a straight runway each night for three nights.

Building large and equal habit strengths.—The primary objective of the first part of the experimental procedure was to build up very large and equal habit strengths for running to both the left and right arms of the single T-maze. One hundred rewarded trials were given on the left side of the maze, 100 on the right. For a given session, one goal arm of the T was removed and 10 forced trials run to the other. The reinforcement consisted of 5 sec. of eating at approximately 24 hr. of drive. The inter-trial interval was 10 sec., including eating time. The direction of forced trials was randomly changed from day to day according to the pattern: RLLRLR-RLRLRLLRLRRLRL. Half the animals received this pattern; half received the opposite. On each day, 10 sec. after the last forced trial, an alternation test trial was conducted by replacing the missing goal arm of the T.

After 100 forced trials (10 days), the running times to both sides had declined to a single asymptotic value which held for the second 100 trials. This was accepted as evidence that very large and equal habit strengths had been built up for running to both sides of the T-maze. On the last 10 days, all animals had alternated with 100 per cent frequency on the alternation test trials, i.e., they always chose the side opposite to that to which they had just previously been forced.

Decay of alternation tendency.—The goal of the next part of the experimental procedure was to introduce varying periods of delay between the series of forced trials and the alternation test trial to determine the rate of decay in time of the alternation tendency. At 10-sec. delay, the previous procedure had established the tendency at a strength of 100 per cent alternation. A delay of 24 hr. was tried by administering the 10 forced trials just before feeding on one night, and giving the alternation test trial on the next. The resulting percentage of alternation was at chance level. This meant that the same animals could be used over and over again at varying delay intervals provided only that a day's rest intervened. A total of eight delay periods was employed. These are given in Table I.

The independent variation of delay period tended to produce concomitant changes in the drive level at the time of the forced trials and/or the time of the alternation test trial. In order to keep drive on these occasions as constant as possible, resort was made to two different experimental sequences. For the long delay periods Sequence A started with the forced trials at 24 hr. of drive, followed immediately by feeding and the onset of the delay period. The alternation test trial terminated the delay period. Sequence B, for the shorter delay periods, started with the daily feeding; later (24 hr. minus the delay period) the forced trials were run followed in order by the delay period and the alternation test trial. The drive levels produced by these schedules are presented in Table I.

TABLE I

SUMMARY OF DRIVE LEVELS AT THE VARIOUS DELAY INTERVALS

Delay Interval* (hr.)	Drive Level at Forced Trials (hr. of depriv.)	Drive Level at Alternation Test Trial (hr. of depriv.)	Experimental Sequence
24	24	24	A
18	24	18	A
12	12, 24	24, 12	A, B
6	18	24	B
4	20	24	B
1	23	24	B
.5	23.5	24	B
0	24	24	B

* The delay interval of zero hours is actually 10 sec. The drive levels given in hours of deprivation should all have 20 min. subtracted for the duration of the feeding period. The amount of food eaten during the forced trials is not regarded as materially affecting the drive strength produced by 24 hr. of deprivation.

The only point at which the lowered drive made a difference in the highly stable running-times of the forced trials was at the 12-hr. drive. As a brief control, at the conclusion of this part of the experiment 12 animals were given a 12-hr. delay test in which they had forced trials run at 24 hr. drive (Sequence A). Eight out of 12 alternated, giving a percentage in good agreement with the final smooth function. This evidence is slight, but taken in conjunction with another fact supports with some st engtth the argument that the variable of drive, concomitant with delays as it is over part of the range, is not responsible for the variation in alternation tendency. This other fact is that the sharpest region of change in alternation percentage lies at the periods of relatively short delay—10 sec., and ½, 1, and 4 hr.—which introduce only slight variations in drive during the forced trials, and consequently no observable difference in performance during these forced trials. It might also be added that there is no obvious theoretical reason why the drive should affect the alternation strength in this way, whereas the theoretical grounds for expecting the delay to do so has already been explicated.

The growth of alternation tendency.—The second half of the experiment had as its objective the determination of the rate of increase rather than decrease of alternation tendency. Ten forced trials in rapid succession had been shown to build up alternation tendency to 100 per cent after a short delay. A series of a smaller number of forced trials was next tried. Using a 1-hr. delay, 1, 2, 4, and 7 forced trials were run with Sequence B. One-hour delay was chosen because it yielded a continuous change in alternation percentage over the range produced by 1 to 10 forced trials.

The only obvious complicating factor would be drive again, varying simultaneously with numbers of forced trials because of the amount of food eaten as reinforcement on each trial. This effect would be slight, however, in view of the very short period (5 sec.) of eating allowed the animal on each trial.

One remaining condition of experimentation should be made explicit in case a question arises over the varying number of alternation tests conducted at the different delay intervals. It was not necessary to follow a rigid experimental schedule of a constant number of animals run every day or every other day. The fact that one day's rest completely eliminated the previous day's experimental effect, coupled with the fact that two days' rest would not materially diminish by retention loss the very large and equal habit strengths on the T, together meant that any convenient number of animals could be run on a given night. The simple policy was adopted of running more tests at those points where the function was undergoing the most change.

RESULTS

Training.—The training administered on the right and left L-shaped runways yielded two learning curves of running-times. The only part of these curves relevant for the present study are the asymptotes, which were long and equal. During the first 50 reinforcements on each side, running-times decreased from an original median value of 12 sec. to an asymptote of 2.0 (±.2) sec. The median of 2 sec. held throughout the remaining 50 trials on each side. The assumption that the large and equal numbers of reinforcements on both sides of the T produced large and equal habit strengths is supported by the constancy of the running-time asymptotes and their equality. Further evidence of the validity of this assumption will be given.

Decay of alternation tendency.—The spontaneous rate of decay in time of

TABLE II

ALTERNATION TENDENCY AS A JOINT FUNCTION OF THE NUMBER OF MASSED, FORCED TRIALS AND THE NUMBER OF HOURS DELAY BEFORE THE ALTERNATION TEST TRIAL

Number of Forced Trials	Number of Hours' Delay	Total No. Rats Alternating / Total No. Test Trials	Percentage Alternation
10	0	200/200	100
10	½	50/56	89
10	1	44/52	85
10	4	38/47	81
10	6	30/46	65
10	12	32/45	71
10	18	27/48	56
10	24	30/59	51
1	1	25/41	61
2	1	31/41	76
4	1	28/41	68
7	1	37/41	90
10	1	44/52	85

the alternation tendency can be seen most readily in Fig. 1, which relates graphically the two variables. The numerical values are presented in Table II.

The alternation percentage starts at 100 after the 10-sec. delay (graphed as zero hours), drops rapidly during the first few hours, and falls more or less constantly thereafter until a chance level is reached between 18 and 24 hr. The over-all course of change is negatively accelerated.

A least-squares method of curve fitting was used to fit a theoretical decay function to the data. The equation of the theoretical curve is

$$\text{Alternation Percentage} = (49 \times 10^{-.061t}) + 50,$$

where t is the time in hours. This curve gives a reasonable fit, as is indicated by the fact that all points but the 12-hr. value deviate from the curve by not more than one standard error of the estimate (4, pp. 301–303).[3] The method of deriving the

[3] The standard error of a proportion used here is probably an attenuated measure of the variability in the general population of animals. This arises from the use of a single animal's data more than once in a given proportion, a procedure which reduces the variability around a proportion that might be due to individual differences in alternation tendency. In using such an attenuated measure of σ_p, we are giving a

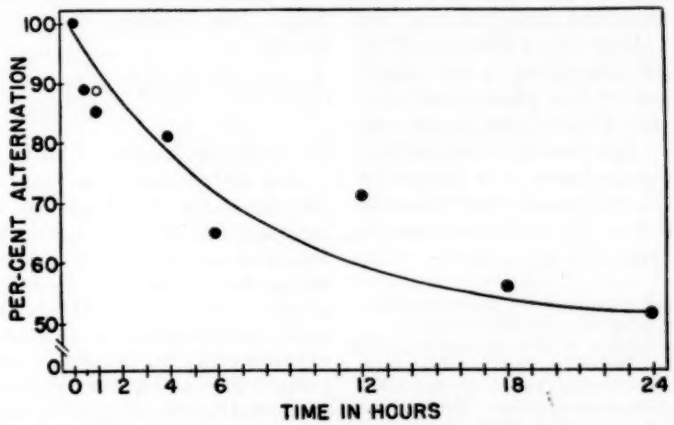

FIG. 1. The rate of decay of alternation tendency after 10 forced responses in massed sequence. The filled circles are the empirical percentages of alternation. The unfilled circle is a semi-empirical point derived from Fig. 2, as explained in the text.

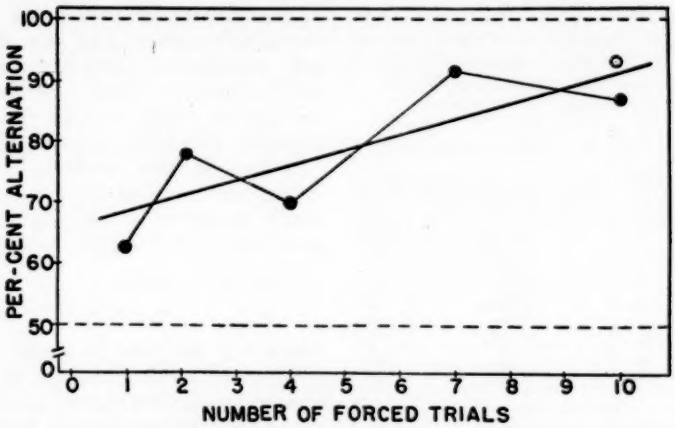

FIG. 2. Alternation tendency after 1-hr. delay, as a function of the number of preceding forced trials given in massed sequence. The dotted lines delimit the range over which the alternation phenomenon can take place. The unfilled circle is a semi-empirical point derived from Fig. 1 and explained in the text.

semi-empirical point is given in the next section.

The growth of alternation tendency.—Figure 2 shows the increase in tendency to alternate at a single choice point as the number of preceding forced trials is increased. The delay period between the series of massed, forced trials and the alternation test is 1 hr. After only 1 forced trial the percentage alternation is not significantly greater than chance, but after 2, 4, 7, and 10 trials, the percentages achieve significance. Computation of the standard error of a proportion (4, pp. 77–78) shows these values to be more than 2.5 σ_p higher than the chance level of 50 per cent.[4]

The theoretically expected function here is linear; so the least-squares method was used to find the best-fitting straight line. Although there appears to be considerable variation of the empirical points around the linear function, all values fall within one standard error of the estimate (σ_{xy}). The equation of the straight line is

$$\text{Alternation Percentage} = 2.6\,N + 64,$$

where N is the number of forced trials.

The 10-trial value is common to Fig. 1 and Fig. 2. A new set of test trials was not run to obtain the empirical point for this function. The best-fitting line estimates a higher value at the 10-trial point. It is this estimate which is carried over to the first graph and plotted as a semi-empirical point. This value is validly termed semi-empirical in that it is weighted by all the empirical points in Fig. 2, and weighted by whatever outside evidence supports the theory calling

conservative estimate of the goodness of fit of the theoretical curve to the empirical points.

[4] The reliability of these proportions being greater than chance has been somewhat overestimated here through use of the attenuated standard error of a proportion. The large size of the last two proportions and the fact that all five are above the chance level are perhaps enough to establish the non-chance nature of the function.

for this equation form. In similar fashion and for the same reasons a semi-empirical point was found by interpolation of the theoretical curve of Fig. 1 at the 1-hr. delay level. This value appears in Fig. 2 at 10 trials, drawn as an unfilled circle. Both of the semi-empirical points fall closer to the theoretical curves, although this was not a necessary outcome of the procedure.

This straight line predicts that it would require 14 forced trials to produce 100 per cent alternation at the 1-hr. delay level. The straight line cannot be extrapolated to the zero forced-trial point, however, because this would have no meaning. With no forced trials preceding the test, we cannot specify what response should be termed "alternation."

Theory and Discussion

The relationship between Hull's theory and alternation tendency has been made explicit by identifying the process with the growth and dissipation of reactive inhibition (I_R). Evidence has been presented in this and previous studies to show that all the qualitative determiners of I_R also affect alternation percentage and in some quantitatively similar ways. The equation forms used to describe the empirical data were dictated by Postulate 8 (6). The constants of these equations, however, may be specific to the given apparatus, *S*s and procedure. In fact, the constant controlling the rate of decay of the alternation percentage makes the process slower than the rate of I_R dissipation usually inferred from other empirical indices. Illustrative examples may be cited to support the point. The rate of spontaneous recovery is theoretically an anchoring process measuring the rate of I_R decay. Miller and Stevenson (8) report the spontaneous recovery of a locomotor response in the rat to be nearly complete in 1 min. Graham and Gagné (3) give evidence of a somewhat slower rate of recovery of a similar response, 3 min. being required to approach an apparent limit. Ellson (2) found the limit of rise of spontaneous recovery of the lever-depressing response to be reached at approximately 3 hr. A rest interval introduced during closely massed acquisition trials produces a spontaneous recovery of performance commonly called reminiscence, another anchoring process for measuring I_R decay. Kimble and Horenstein (7) performed a reminiscence experiment with human subjects on a pursuit rotor task and inferred from their data that the I_R generated in that situation was completely lost in 10 min. of rest. Of the host of studies demonstrating the superiority of distributed over massed learning trials, the effect has usually been shown when the variable of inter-trial interval is of the order of magnitude of minutes rather than hours. All of these data, while not highly consistent with each other, may be contrasted with the findings of the present experiment in a single respect, namely, that the inferred rate of decay of I_R is faster than decay of alternation tendency after 10 forced responses.

Two possible explanations of this will be considered. The first states simply that there is an inhibiting factor involved in the alternation situation which resembles reactive inhibition in that it has all of I_R's qualitative determiners, decays spontaneously and with negative acceleration, but which dissipates at a rate somewhere between that of the relatively volatile I_R and the more permanent ${}_sI_R$. The consequences of such an explanation, while directly deduci-

ble, are largely programmatic. It would be necessary to collect large numbers of inferred I_R decay curves from the wide variety of empirical I_R anchors, attempt to fit a single equation form to these curves, and then make a distribution of the rate-of-decay constants (q). The above explanation would predict a bi-modal distribution of q's. The hypothesis might also be tested by a small program of research within the area of alternation experiments. It will be recalled that Heathers and others found the decay of alternation percentage to be a matter of minutes when only a single response preceded the alternation test trial, in contrast to the many hours needed in the present experiment when 10 massed forced trials preceded the alternation test trial. The question arises at what rates will alternation percentage decay when varying numbers (between 1 and 10) of forced trials are tried. How much of the fast decaying I_R must one build before the slower decaying stuff is obtained? The experimental answer to this question is presently under investigation by B. J. House. If the above two-kinds-of-I_R hypothesis is true, the varying rates of decay of alternation percentage after varying numbers of forced trials will divide themselves dichotomously. A simpler outcome may be the finding of a variety of rates, the rate being some function of the amount of I_R. An equation describing this manner of decay could easily be found to replace the less complex one now employed by Hull.

A second possible explanation of our results states that the alternation situation provides a very sensitive index of the presence of I_R not usually provided by other experimental operations and performance measures. This would account for the detection by this method of small amounts of I_R long after other methods and measures had become insensitive and thus account for the slow rate of decay of I_R. At least one line of evidence supports this explanation, and this concerns the observed, relative insensitivity of other performance measures of I_R in the present experiment. Figure 2 shows that for the short delay group the rats were alternating with a 100 per cent frequency after 10 forced trials, a very strong preference. Yet a comparison of the running speeds on the last forced trial with those of the alternation trial shows no significant difference. By the "choice" measure, going left was 100 per cent dominant over right; by running-speed index both tendencies were the same.

One theoretical translation of this explanation may be given here. I_R is linked to observable behavior through effective reaction potential with its four measures, resistance to extinction, latency, amplitude, and probability (p) of response. The measure we are dealing with here is probability (p), or strictly, the probability difference of two very strong mutually exclusive, super-threshold responses, turning left and turning right. According to Hull (6, p. 333), the probability of an all-or-none type of response is not a sensitive index of ${}_s\bar{E}_R$ at the high and low values of ${}_s\bar{E}_R$. This implies that the curve relating p and ${}_s\bar{E}_R$ is ogival in shape. The explanation under translation here requires that there be more than one relationship between p and ${}_s\bar{E}_R$ depending upon the nature of the experimental situation. Specifically, the assumption is needed that p is some sharply rising function of ${}_s\bar{E}_R$ when the probability of response is determined by slight differences in very strong and competing ${}_s\bar{E}_R$'s.

Add to this assumption the statement that the present alternation situation, providing asymptotic ${}_{S}E_{R}$'s and ${}_{S}I_{R}$'s, makes any difference between the right and left turning effective reaction potentials attributable to I_R, and the theoretical translation of an "I_R sensitive situation" is explicated.

One consequence of this interpretation is at variance with the first explanation given above, and hence an easy test may be made between them. If the sensitivity view is correct, different amounts of I_R should dissipate at the same rate constant (q) when measured by alternation tendency. A simple test discriminating between these two explanations will be made by investigating by alternation technique the relation between q and N (number of forced trials). If q varies, the first explanation is supported; if it remains constant, the second tends to be confirmed.

As a final theoretical consideration, the relation between our data and Solomon's modification of Hull's inhibition concepts will be examined. Solomon replaces I_R with the more "peripheral" construct of kinesthetic stimulation acting as cue and/or drive in order to achieve the following advantages: "(1) the explanatory construct employed is ultimately capable of independent (and direct) measurement; (2) the results of responding are broadened to include the cue aspects of response-produced effects; and (3) the deductions generated are more comprehensive, with several physiological implications" (**10**, p. 5).

Our data have some bearing upon the physiological implications. The identification of I_R with kinesthetic drive and cue stimuli makes the decay of I_R analogous to the fading of kinesthetic traces. The temporal course of decay of both processes should be of the same order of magnitude if the identity is valid. Neural after-discharges in single fibers disappear in a matter of milliseconds. More complex neural networks of the reverberatory type are known, however, which are capable of sustaining for longer periods of time the neural traces of past stimulation. I_R as measured by many behavioral indices dissipates in a matter of minutes, a duration suggesting the mediation by the more complex type of neural after-discharge. I_R as measured by the alternation tendency in the present experiment extends the decay period to a number of hours. This means that we have some evidence that neural networks reverberate for hours, if alternation does measure I_R, and if I_R is identified with kinesthetic stimuli. What further behavioral consequences there are of this presumptive physiological evidence, we are unable to state.

About the second advantage of peripheral constructs enumerated above, the following comments appear pertinent. Hull's system does not need to be broadened to include the cue aspects of response-produced effects. It does so already. Postulate 6 (**6**, p. 253) states that associated with every drive (D) is a characteristic drive stimulus (S_D). Postulate 8 (**6**, p. 300) begins with the statement that responding creates as a result a primary negative drive (D) having an innate capacity (I_R) to inhibit further responding. From these two statements it can easily be deduced that there would be an S_D associated with every I_R state which could serve as a cue if differentially reinforced.

And about the first advantage enumerated above we have these concluding remarks. While it is true that a kinesthetic stimulus can be measured physiologically, the cue and drive (explanatory) properties of the kinesthetic stimulus cannot. They,

like I_R, must be inferred from S–R relationships. The peripheral constructs may have a physiological anchor (as yet undiscovered) which the Hullian constructs do not, but neither of these are capable of direct independent measurement.

Summary

An experiment was performed to measure the tendency of rats to alternate their responses on a single T-maze after varying numbers of forced trials to one side of the T. Prior to the measurement of alternation tendency responses to both sides of the T were given large numbers of reinforcements to build equal and asymptotic habit strengths. The percentage of rats alternating one hour after 1, 2, 4, 7, and 10 forced trials was determined. Also determined was the relationship between the percentage of alternation after 10 forced trials and interpolated delay periods of 0, ½, 1, 4, 6, 12, 18, and 24 hr.

The following results were obtained:

1. The percentage of rats alternating increases linearly with number of forced trials.

2. Alternation percentage decreases as a negatively accelerated function of the length of delay period after 10 forced trials.

The following theoretical interpretations were made:

1. Previous findings have supported the identification of alternation percentage with Hull's theoretical variable I_R. This identification makes possible the prediction by Hull's Postulate 8 of the equation forms relating alternation percentage to number of forced trials and length of delay period.

2. The temporal course of decay of I_R, although of theoretically expected shape, was found to be considerably slower than that inferred from other empirical indices of I_R.

3. Two alternative theoretical explanations of the discrepancy were offered and some further research outlined for deciding between them.

4. The identification of I_R with kinesthetic stimulation was discussed.

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EFFECTS OF PRE-PRACTICE ACTIVITIES ON ROTARY PURSUIT PERFORMANCE [1]

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In analyzing the rotary pursuit performance curve quantitatively, Ammons (1) defined certain variables including temporary work decrement and warm-up decrement. "Warm-up decrement" was defined as the amount that the mean performance level on the first post-rest trial was depressed below a point determined by backward extrapolation of a line fitted to the mean performance levels on several subsequent trials, those in the relatively decremental segment of the curve. It was shown that this need to warm up became less as the *S* had more experience starting practice, and it was predicted that there might well be almost no warm-up decrement in instances where practice had been initiated many times. This prediction has been found to hold in one study (2) of distribution of practice. Several groups which had started practice on 36 different occasions separated by rests of 2 or more min. showed no appreciable warm-up decrement after a rest 12 to 30 min. in length. These findings justify the backward extrapolation of the subsequent portion of the post-rest performance curve to correct for warm-up decrement in the first minute or two of post-rest practice.

Amount of temporary work decrement was defined as the difference between performance on the first trial after a 10-min. or longer rest, corrected for warm-up decrement, and estimated performance on that trial if no rest had been introduced. Actually, this difference represents *recovery* from temporary work decrement.

In the original theoretical treatment of warm-up decrement, it was tentatively explained as due to loss over rest of "set," or postural and sense organ adjustments favorable to performance. Irion in a recent article (4) reports that if the first 24-hr. recall trial of a paired-associate series is preceded by a period of naming color strips presented on a memory drum in a manner similar to that used with the paired associates, there is significantly better performance than where no such activity precedes the trial. He proposes that much of what is ordinarily lost over rest is "set," and that the color-naming practice reinstates it, minimizing the effects of its loss on recall.

Problem

Following this line of reasoning, it can be predicted that: (*a*) Practice of various components of the rotary pursuit task immediately before the initial practice period will increase subsequent performance, since set responses will be learned, giving a basis for higher scores. (*b*) Practice of various major components of the task immediately before practice is resumed after a rest should decrease the warm-up decrement.[2]

[1] Thanks are due Mr. Wilbur Hubbard of Oregon State College and Mr. Cliff Wing of Tulane University for assistance in collecting data. This study was made possible by research grants from the Bureau of Humanities and Social Science Research of the University of Denver (1947–48) and the Carnegie Foundation through Tulane University (1948–49).

[2] Similar predictions are made by Irion (3) in his review of the relation of "set" to retention in verbal learning.

METHOD

Materials.—Ss practiced on Koerth-type pursuit rotors,[3] with ¾-in. brass targets with centers set 3½ in. out from the center of 10 ¾-in. bakelite disks and flush with their surfaces. Target plates rotated in a horizontal plane at 60 rpm. The targets were followed during regular practice with the tips of light hinged styluses, and electrical contacts between targets and styluses were recorded cumulatively on timers during 20-sec. periods. Scores were read from the clocks at the end of each 20-sec. period, although subsequent treatment of the data was in terms of 1-min. periods. Timing of trials was manual.

Subjects.—The *Ss* were experimentally naive undergraduate women college students attending the University of Denver or Colorado Woman's College. Although 28 were assigned randomly in groups of four to each of the eight conditions, difficulties with apparatus, instructions, or interruptions led to the discarding of a total of 22 records. Final *N*'s for the conditions are given in Table I. It will be seen that they varied from 24 to 28.

Prodceure.—The *Ss* practiced in groups of four. Each was assigned a rotor. Instructions were then given as follows: The purpose of the study was to find out how people learn a new task; they would find that they would not do too well to begin with but as they kept at it, they would improve; the best way to practice was with body relaxed, following the target with an easy swinging motion of the arm; each was to use her preferred hand to hold the stylus; her grip was to be light, and no attempt was to be made to put extra pressure on the tip of the stylus. The *Ss*' grip and stance were then checked and corrected where necessary. A ready signal was given, and pursuit began approximately 5 sec. later.

All *Ss* practiced 12 min. continuously, ceased practice for 17 min., then practiced continuously for 12 min. more. They were encouraged periodically and were corrected verbally when they deviated from instructions in stance or stylus grip. The eight conditions are described briefly in Table I. It will be seen that provision was made for measuring the effects of manual or ocular practice for 2 min. immediately before each session or both sessions. Imaginary practice could only be given meaningfully after *S* had already had some experience with the task.

Ocular practice consisted of *S*'s standing for 2 min., following the target with her eyes only. In manual practice, *S*'s eyes were blindfolded, and she was shown how to follow with her index finger a small rivet head set into the rotor plate the same distance from the center as the center of the regular target. When she lost it, she attempted to find it again and to resume following. No *S* was unable to do this. For imaginary practice, *S* stood before the rotor with eyes blindfolded. She was told to imagine that she was practicing, without, however, making any overt movements. This instruction was continually reinforced with suggested details. It was judged effective in that two *Ss* showed trance-like behavior in raising their forearms into practice position and were discarded, and all remaining *Ss* in this group reported being able to imagine the practice quite clearly.

During the rest period, *Ss* sat in an anteroom and talked or read popular magazines. At the end of each practice period, the rotors were turned off promptly and *Ss* laid down their styluses.

[3] This apparatus has been described in detail elsewhere (2).

TABLE I

SUMMARY OF EXPERIMENTAL CONDITIONS

Condition	*N*	Description
I	28	Control with two 12-min. continuous practice periods separated by a 17-min. rest
II	27	2-min. ocular practice before* initial practice period
III	24	2-min. ocular practice before* both practice periods
IV	24	2-min. ocular practice before* final practice period
V	24	2-min. blindfolded manual practice before* initial practice period
VI	26	2-min. blindfolded manual practice before* both practice periods
VII	24	2-min. blindfolded manual practice before* final practice period
VIII	25	2-min. imaginary practice before* final practice period

* Delay between cessation of pre-practice activity and start of regular session was 20 sec. or less.

RESULTS

Curves showing mean performances for the various groups are given in Fig. 1. It is immediately apparent from inspection that all curves show recovery from temporary work decrement over rest, post-rest warm-up decrement, and a decremental segment in the post-rest curve, presumably because temporary work decrement is building up again. It is also apparent

that there are no great differences between the various groups in pre- and post-rest performance.

To evaluate the results statistically, scores at various points in practice were used as indices as follows: (*a*) first minute of pre-rest practice, to indicate effects of pre-practice on initial level of performance; (*b*) last minute of pre-rest practice, to indicate effects on final level of performance reached in pre-rest practice; (*c*) first minute of post-rest practice, to indicate effects on warm-up decrement;

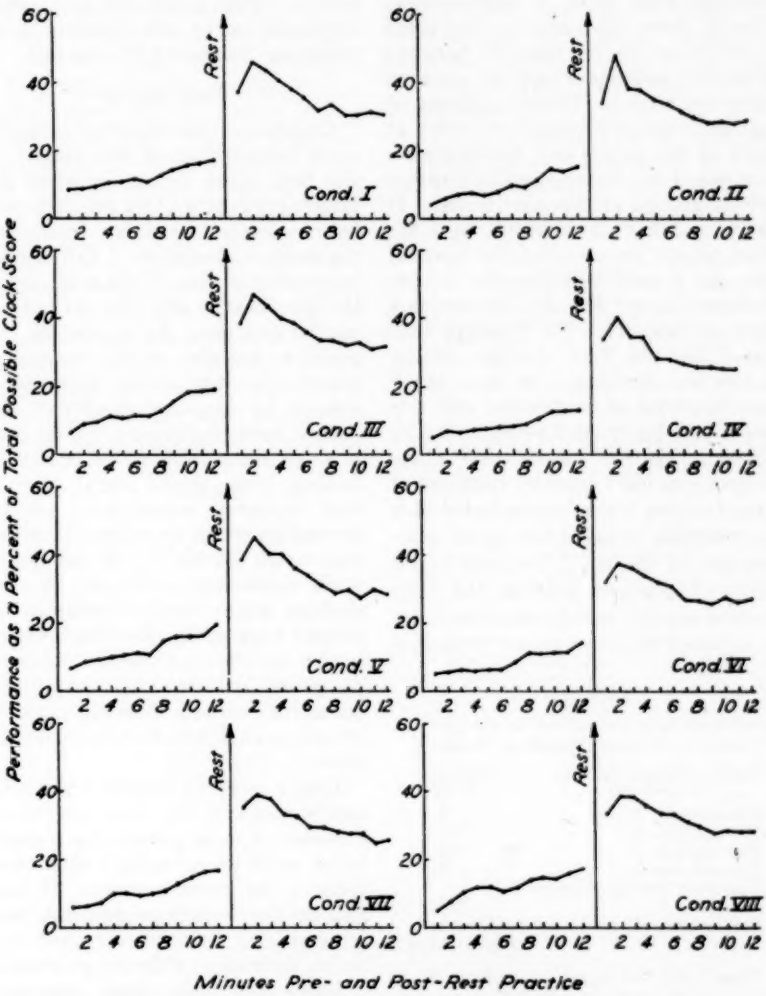

FIG. 1. Pre- and post-rest mean performance under the eight pre-practice conditions

(*d*) difference between first and second minutes of post-rest practice, to obtain a second estimate of effects on warm-up decrement; (*e*) second minute of post-rest practice, to indicate reminiscence or recovery from temporary work decrement over rest; (*f*) last minute of post-rest practice, to indicate final level of performance. Use of index (*d*) assumes that there is little or no relationship between warm-up decrement and the score on post-rest trial 2. Simple analyses of variance were performed on scores at each of the points and *F*-ratios were computed for between-groups versus within-groups variance estimates. It is realized that the distributions in the first minute are considerably skewed, but the *F*-ratio still provides a convenient index. Results are summarized in Table II. No *F* except that based on the first pre-rest minute scores was significant at even the 5 per cent level of confidence, and this one is here interpreted as occurring by chance. To the extent that these indices provide adequate measures of the variables, it can be concluded that pre-practice, imaginative or on components of the skill, has little or no effect on practice level at the very start of regular pursuit rotor practice, on recovery from temporary work decrement or warm-up decrement, or on final level of performance.

TABLE II

RESULTS OF SIMPLE ANALYSIS OF VARIANCE OF SCORES AT VARIOUS POINTS IN PRACTICE

Source of Variance Estimate: Scores from	Obtained *F* Ratio*
1. Pre-rest min. 1	2.53
2. Pre-rest min. 12	1.77
3. Post-rest min. 1	1.00
4. Post-rest min. 2	1.39
5. Difference between post-rest min. 1 and 2	1.40
6. Post-rest min. 12	(Error term slightly larger)

* For 7 and 194 degrees of freedom, the *F* ratio must be approximately 2.73 to be significant at the 1 per cent level of confidence (6).

On the other hand, a careful inspection of the original data suggests that imaginary practice may possibly have had an appreciable effect not detected by the present statistics. In condition VIII (imaginary practice) recovery from temporary work decrement was less by about one-third, and warm-up decrement by one-half.

DISCUSSION

Comments seem in order regarding some implications of this study. In the first place, it can be seen that Irion's prediction of set reinstatement by pre-practice does not hold under the present conditions. This is quite impressive in view of the similarity of the pre-practice activities to those in regular practice. It is possible that positive transfer from component practice is just about matched in amount by negative transfer of temporary work decrement. This guess is given some support by Irion's (5) finding with verbal serial learning that pre-recall warming-up activity beyond an optimum amount may produce work inhibition. If temporary work decrement produced by pre-practice were a major factor in the present experiment, one would expect to find significant differences between the groups at least at post-rest trial 2. Its relative unimportance is indicated by the small *F* based on scores at this point.

In any case, if component pre-practice is to have any considerable importance, it must perhaps be engaged in for much longer periods than those used in the present study. It may also be that rotary-pursuit and nonsense-syllable learning sets will turn out to have quite different properties.

There has been some discussion about the nature of temporary work

decrement. On the basis of the present study it can be concluded that it is not directly dependent upon the rate or amount of energy expenditure, since 2 min. of manual pre-practice produced, at most, only a small amount of the effect the same amount of work would have had if *S* had had her eyes open and been following the target. It would seem that temporary work decrement is mainly central in nature rather than peripheral.

The results of the present study throw some light on the rehearsal hypothesis of reminiscence. Activities comparable to rehearsal were actually encouraged during the "rest" period. If anything, post-rest performance was poorer as a result. Taken in conjunction with similar results reported by Rohrer (7) for verbal material, this study still further discredits the hypothesis that *Ss* do better after rest because they have somehow rehearsed when they were supposed to be resting.

Summary and Conclusions

Two 12-min. practice periods of continuous rotary pursuit, separated by a 17-min. rest period were given 202 undergraduate college women. Groups practiced under eight different conditions of pre-practice warming-up activity, consisting of ocular following of the target, blindfolded following of a rivet head, or imagined practice. Pre-practice was given before one or both of the practice periods. Statistically significant score differences between groups appeared at no point in practice.

It is concluded that the present types and durations of pre-practice activity do not produce significant changes in subsequent performance level, warm-up decrement, or temporary work decrement. These results do not support an energy-expenditure explanation of temporary work decrement, or a rehearsal theory of reminiscence. They do seem to indicate that verbal and motor performances are affected differently by pre-practice warming-up activities.

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A COMPARISON OF TRANSFER EFFECTS DURING ACQUISITION AND EXTINCTION OF TWO INSTRUMENTAL RESPONSES [1]

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In an earlier paper (6) it was suggested that experimental extinction may be expected to transfer[2] more broadly than acquisitive learning, the suggestion having been made on the basis that extinction procedures usually provide very little differential reinforcement by comparison with the procedures of acquisition. In the case of an instrumental habit, for example, the procedures of acquisition are such that reinforcement is given only on the occurrence of the "correct" response; the many incorrect responses are not reinforced; and the learning animal is therefore required to isolate the behavior which is correct in any given situation from that which is not. When that same instrumental habit is being extinguished, the particular reward which had earlier been given for the correct response is now given for none of the responses which may occur, with the result that the animal is not encouraged to discriminate among specific responses or among specific tendencies not to respond. If it may be assumed, additionally, that breadth of transfer depends largely on the extent to which the learning situation requires that discriminations be made, then it is to be expected that the extinction of a habit will produce a transfer effect far broader than that which will have occurred while that same habit was being acquired.

These assumptions about the transfer of extinction have been useful in an attempt to account for spontaneous recovery (5, 6), especially in dealing with the reduction in recovery which follows differential extinction (6), and to that extent their validity has been indirectly established. In this paper the attempt will be made to provide a more nearly direct test for the one general assumption that extinction transfers more broadly than acquisition.

The literature provides only a few data relevant to a comparison of transfer effects during acquisition and extinction; moreover, such data as are available were collected more or less incidentally by experimenters who were not primarily concerned to make the comparison, and, for the most part, the data are not critical for our purpose. In the case of instrumental habits, with which category this paper will be exclusively concerned, two experiments provide reasonably pertinent information.

One experiment, by Youtz (10), showed a very great transfer of extinction between vertical and horizontal bar-pressing, but there was also, apparently, some transfer between these habits during acquisition. Youtz did not try to compare the transfer of acquisition with the transfer of extinction, and, indeed, such a comparison would have been hard to make, since the units which were used to

[1] This study was carried out at Wesleyan University with the very able assistance of Richard A. Otico.

[2] Unless otherwise specified, "transfer" will here mean positive transfer, and will be defined by the extent to which training on one habit facilitates training on another, or extinction of one habit facilitates extinction of another.

measure the two transfer effects are not easily equated. It may be relevant to that comparison, however, that there was a large transfer of extinction from vertical to horizontal and from horizontal to vertical, whereas the transfer effect during acquisition was apparent only in the case of vertical to horizontal.

Using the same habits of horizontal and vertical bar-pressing, Williams (**8, 9**) found that the transfer effect during acquisition was positive, negative, or virtually zero, depending on the relative amounts of training on the two habits. In extinction, however, there was a reliable transfer effect for all amounts of training.

The purpose of the present experiment is simply to permit a more nearly direct comparison of the transfer effects which may occur during acquisition and extinction of two rather dissimilar instrumental responses, in this case bar-pressing and a running response, and thus provide more and, perhaps, clearer evidence for the assumption that the transfer effects of extinction are typically broader than those which occur during acquisition.

Procedure

Two groups of 23 rats each were trained and then extinguished on a runway and a modified Skinner Box. To provide measures of transfer effects during acquisition and extinction, one group was trained first on the runway and then on the Skinner Box and extinguished on the two tasks in the same order, while the second group was trained, and also extinguished, on the two tasks in reverse order.

The runway was the same as that described in a previous article (**6**). It was L-shaped, about 6 in. wide, with sides 4 in. high. The longer arm of the L was about 4½ ft. in length and had at one end a starting area which could be separated from the rest of the runway by a drop shutter. The shorter arm of the L was approximately 3 ft. long and terminated in a compartment 1 ft. square. Hidden behind a partition in one corner of the food compartment was the food dish, so arranged that its contents could not be seen by a rat from any position behind a "finish" line drawn from the partition to the side of the compartment.

The modified Skinner Box was 10 in. square and 9 in. high, inside, with a coarse wire mesh covering the top of the box. A small retractable bar extended into the box from a point about 2 in. above the floor and was arranged in connection with a food delivery device so that depression of the bar would cause a small pellet of food to be dropped into a tray directly below the bar.

The *Ss* used in this experiment were male albino rats between 3 and 4 months of age, purchased from the Albino Farms, Red Bank, N.J.

The specific procedure was as follows. For 12 days prior to the beginning of the experiment proper each rat was given treatment calculated to habituate it to the procedures of the experiment and to build up a moderately strong hunger motivation. On each of the first 5 days of the pre-experimental treatment the rat was simply picked up and petted by the experimenter four or five times daily. Beginning with the sixth day and continuing to the end of the experiment, the rat was fed each day an amount of food equal to about two-thirds of the daily requirement. On the seventh day the rat was taken from its individual living cage, placed in the runway, permitted to explore for 1 min., and then put back into the living cage. This procedure was repeated three times on the runway, and then three comparable trials were given on the Skinner Box. On the eighth day 4 habituation trials were given on the Skinner Box and the runway; 5 habituation trials on each were given on the ninth day, 6 on the tenth day, 7 on the eleventh day, and 8 on the twelfth day. On the eleventh and twelfth days the rat was fed one 0.08-gm. pellet of food on each habituation trial on the runway and Skinner Box, the food being placed at randomly determined positions on the floor of the runway and Skinner Box. On all habituation trials in the runway the square compartment (containing the food dish) was blocked off, and the shutter for the starting box was never in place; in the Skinner Box the bar and food tray were withdrawn.

Training on both runway and Skinner Box was given on the day immediately following the twelfth and last day of the pre-experimental treatment. On each training trial on the runway the rat was taken from its living cage and placed in the starting box of the runway. About 1.5 sec. thereafter, the shutter was raised and the rat was permitted to make its way to the food dish where it found and consumed a 0.08-gm. pellet of dog chow. The rat was then placed in its living cage for 1 min., after which it was put into the runway for the next trial. To secure a measure of the rat's performance, a watch was

started when the starting box shutter was raised, and stopped when *S* crossed the "finish" line in front of the food dish. That time interval will be called the "latency" of the running response.

On each training trial in the Skinner Box the rat was taken out of its living cage and placed in the Skinner Box, where it remained until it had pressed the bar and received and eaten the 0.08-gm. pellet of food. The bar was retracted immediately after it had been pressed, and remained retracted for the duration of the trial. Following each trial on the Skinner Box, the rat was put back into its living cage for 1 min. Performance in the Skinner Box was measured as the time taken by the rat to press the bar, and this amount of time will be called the "latency" of the bar-pressing response.

One group of 24 rats (to be called Group I) was given eight consecutive trials on the runway, followed after 1 min. by eight consecutive trials in the Skinner Box. A second group of 24 rats (Group II) received eight consecutive trials on the Skinner Box, followed after 1 min. by eight consecutive trials on the runway.

Extinction procedures were applied on the day following training. The rats in Group I (trained on runway first and Skinner Box second) were given eight consecutive nonreinforced trials on the runway, followed within a period of 1 min. by eight consecutive nonreinforced trials on the Skinner Box. Rats in Group II (trained on Skinner Box first and runway second) were given eight nonreinforced trials on the Skinner Box, followed after 1 min. by eight nonreinforced trials on the runway. On all extinction trials the rat was taken from the apparatus and placed in the living cage within 20 sec. after it had made the critical response (crossing the finish line in the case of the runway and depressing the bar in the case of the Skinner Box). The rat was given a maximum of 4 min. to make the response. If the response did not occur within that time, the rat was removed from the Skinner Box or runway and put into the living cage in preparation for the next trial. (The interval between the end of one trial and the beginning of the next was 1 min.)

Results

The individual response latencies recorded in the runway and Skinner Box have been transformed into logarithms in order to normalize a distribution of measures which shows otherwise a marked skewness toward the high values. A logarithmic transformation has been used and justified by Graham and Gagné (**2**) for data

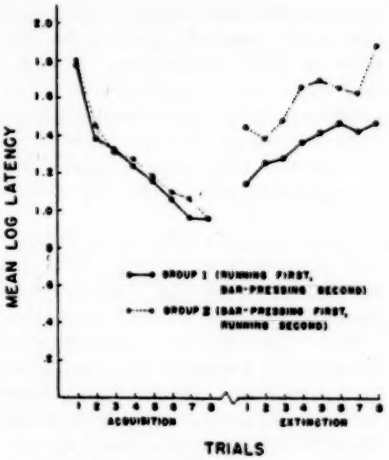

Fig. 1. Mean log latencies for acquisition and extinction of the running response

altogether comparable to those being reported here, and a more complete account of the rationale for such a conversion is to be found in a recent article by Mueller (**7**).

Figure 1 describes the performance of all animals during training and extinction on the runway. The values indicated by the solid circles are the mean log latencies for the 24 rats of Group I, which received training on the runway prior to training in the Skinner Box, and which (on the day following training) were extinguished on the runway prior to extinction in the Skinner Box. The open circles are the mean log latencies for the 24 rats of Group II, this being the group which had its training on the runway immediately after it had received eight trials in the Skinner Box.

The curves of acquisition for the two groups are obviously very similar, indicating that there was no transfer of training from bar-pressing to the running response. In the course of extinction, on the other hand, the latencies are considerably greater for

Group II, which was extinguished on the running response after having had eight trials of extinction on bar-pressing, and it would appear, therefore, that extinction of bar-pressing transferred to running. The results of an analysis of variance,[3] to be found in Table I, confirm these conclusions.

During acquisition the between-groups variance is insignificant (F is less than 1), which means that the groups do not differ on the eight trials of training taken as a whole. It may also be relevant to any comparison of the two groups to note that the interaction between trials and groups is insignificant, and that there is, accordingly, no evidence that the acquisition curves differ in slope or curvature. Trials contribute a significant variance, as one would expect in a situation in which learning is presumably taking place.

[3] The writer is much indebted to Dr. Geoffrey Beall for suggesting how the analysis of variance should be applied to these data. Similar analyses of comparable data are to be found in Kogan (4) and Edwards (1).

In extinction the between-groups variance is highly significant, and we may, therefore, have all the more confidence in that difference between the groups which is seen so clearly in Fig. 1. The variance attributable to trials, which is highly significant, simply reflects the fact that extinction occurred in both groups.

The difference in log latency between the last trial of training and the first trial of extinction measures the forgetting which occurred over the 24-hr. interval between the training and extinction sessions.

In Fig. 2 are the mean log latencies which describe for both groups the acquisition and extinction of the bar-pressing habit. The acquisition curves, in this case, would seem to show that the effect of prior training on the runway (Group I) was to reduce the speed of responding during training on the bar-pressing, particularly in the later stages, but the analysis of variance for the training data (Table II) fails to reveal a sig-

TABLE I

ANALYSIS OF VARIANCE OF THE LOG LATENCIES FOR ACQUISITION AND EXTINCTION OF THE RUNNING RESPONSE

Source of Variance	Sum of Squares	*df*	Mean Square	*F**	*P*
Acquisition					
Trials	23.9388	7	3.4198	48.58	<.01
Between groups	0.2123	1	0.2123	<1	
Between subjects in the same group	14.4706	46	0.3146		
Interaction: trials × between groups	0.1315	7	0.0188	<1	
Interaction: trials × between subjects in the same group	22.6556	322	0.0704		
Extinction					
Trials	5.9495	7	0.8499	7.13	<.01
Between groups	6.0802	1	6.0802	8.60	<.01
Between subjects in the same group	32.5344	46	0.7073		
Interaction: trials × between groups	0.6837	7	0.0977	<1	
Interaction: trials × between subjects in the same group	38.3872	322	0.1192		

* In determining F for the between-groups variance, the error term was between-subjects-in-the-same-group; for the interaction between trials and groups, the error term was the interaction of trials and between-subjects-in-the-same-group; and for trials, the error term was the interaction of trials and between-subjects-in-the-same-group.

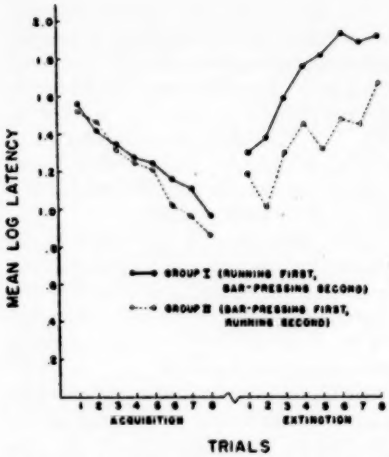

FIG. 2. Mean log latencies for acquisition and extinction of the bar-pressing response

nificant difference in performance between the two groups. One sees, also, in Table II that the practice × groups interaction is too small to support the assumption that the acquisition curves for the two groups are anything but parallel.

It is reasonably clear from the extinction curves of Fig. 2 that extinction effects transferred from running response to bar-pressing, inasmuch as the latencies are considerably greater throughout extinction for the group (I) which had been given eight trials of extinction on the runway prior to extinction on the Skinner Box, and the analysis of variance in Table II shows that the difference between the groups is highly significant.

DISCUSSION

Training on the habit which was learned first did not facilitate or significantly inhibit the learning of the second habit, but the extinction of either of these habits did facilitate the extinction of the other. Thus, it would appear that the transfer of extinction was broader than transfer during acquisition. We must, of course, qualify that conclusion to take account of the possibility that the absence of any transfer effect during acquisition is to be attributed to the simultaneous operation of inhibiting and facilitating factors. If these two factors developed equally during train-

TABLE II

ANALYSIS OF VARIANCE OF THE LOG LATENCIES FOR ACQUISITION AND EXTINCTION OF THE BAR-PRESSING RESPONSE

Source of Variance	Sum of Squares	*df*	Mean Square	*F*	*P*
Acquisition					
Trials	15.4894	7	2.2128	19.24	<.01
Between groups	0.3326	1	0.3326	<1	
Between subjects in the same group	20.9275	46	0.4549		
Interaction: trials × between groups	0.2929	7	0.0418	<1	
Interaction: trials × between subjects in the same group	37.0424	322	0.1150		
Extinction					
Trials	15.7011	7	2.2430	10.86	<.01
Between groups	11.0331	1	11.0331	9.46	<.01
Between subjects in the same group	53.6796	46	1.1669		
Interaction: trials × between groups	1.3928	7	0.1990	<1	
Interaction: trials × between subjects in the same group	66.5149	322	0.2066		

ing on the first task, and transferred equally to the second, the final transfer effect would be an algebraic sum of approximately zero. Though such an interpretation might seem rather implausible, there is nothing in the procedure or data of this experiment which clearly excludes it as a possibility.

There is some evidence that the effect of training on the first task was to inhibit the learning of the second. The analysis of variance revealed no statistically significant transfer effects during acquisition, but an inspection of the acquisition curves shows that in bar-pressing and also in running the latencies were somewhat greater, on most of the trials, for the group which had previously been trained on another task. If these differences were reliable, they would be assumed to reflect the operation of some factor like fatigue or reactive inhibition which produces a performance decrement even when responses are reinforced, and which, in this case, accumulated in the course of training on the first task and then transferred to the second. This same kind of fatigue would be expected to develop during extinction, and its transfer from the one task to the other might conceivably have been responsible for the transfer effect in extinction. It should be noted, however, that such an assumption does not, by itself, account for the fact that transfer was greater in extinction than in acquisition. We should suppose that the fatigue or reactive inhibition is produced as a result of work done, and simply subtracts from habit strength to produce performance. A rat which has completed eight reinforced trials on a particular task has presumably the same amount of fatigue as a rat which has completed eight unreinforced trials on the same task. In our experiment, therefore, the difference in fatigue between the groups during acquisition should have been equal to the difference between them during extinction. The fact that the groups differ in performance during extinction and not during acquisition must then mean, according to a fatigue interpretation, that the fatigue which develops during extinction transfers more broadly than the fatigue which develops during training.

It is also relevant to an interpretation of our results that the procedures of this experiment were such as to reduce, if not to eliminate, the kind of transfer effect which results when inexperienced or unhabituated animals are used. In this connection Jackson (3) has shown that such factors as (1) habituation to experimental diet and feeding schedule, (2) habituation to being handled, and (3) "development of a general tendency to explore an apparatus vigorously" could separately or together produce positive transfer effects, and these three factors were assumed to account for the positive transfer effects which were obtained in part of Jackson's study between a problem box and a maze. With regard to the present experiment it was felt that a more generally meaningful comparison of transfer of training and extinction could be made under conditions in which the transfer effect resulting from the above-mentioned factors was largely excluded by carefully habituating the animal to virtually all aspects of the experimental procedure save the specific instrumental responses to be learned.

Summary

The experiment reported here was designed to provide evidence bearing on the assumption that the extinction of a particular habit will produce transfer effects broader than those

which will have occurred while that same habit was being acquired.

One group of rats was trained first to run down a runway and then to press a bar in a modified Skinner Box, and, on the day following training, that group was extinguished first on the running response and then on the bar-pressing. A second group was trained, and also extinguished, on the two habits in reverse order.

Training on the bar-pressing response did not affect subsequent learning of the running response, nor did training on the running response significantly affect learning of the bar-pressing. The extinction of either habit, on the other hand, considerably facilitated the extinction of the other.

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SIMILARITY IN STIMULATING CONDITIONS AS A VARIABLE IN RETROACTIVE INHIBITION

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Current theories of retroactive inhibition stress the interference which interpolated activity exerts on the recall of the original material. Hence, the greater the similarity between original and interpolated learning, the more difficult it should be for *S* to discriminate between the two sets of materials, and the greater the retroactive inhibition. This has been adequately demonstrated for similarity between the actual materials; the case is less clear for similarity of incidental stimulation (3). Bunch and McTeer (2) found less retroactive inhibition between two mazes when one of them was accompanied by shock. But Nagge, using nonsense syllables, found no decrease in retroactive inhibition when the interpolated list was learned in a room which contrasted with that employed during original learning and recall (4). Since theory would lead us to believe that incidental stimulation from room, posture, and apparatus might serve as discriminative stimuli, thus decreasing confusion between original and interpolated learning, Nagge's experiment was repeated. It will be shown that the use of obtrusive differences between the conditions under which original and interpolated lists were learned leads to positive results.

[1] This research is reported in considerably greater detail in the junior author's thesis (1), available through inter-library loan.

METHOD

Apparatus.—Two experimental situations were made as different as possible. The *card room* was a large basement classroom with light cream-colored walls, a blackboard, and tablet armchairs. In it was a card-flipping device that worked like a desk calendar. The cards were 5½ by 4½ in., carrying words printed in ¼-in. letters. The *S* sat in front of the exposure apparatus, while *E* flipped the cards in synchronism with a click which occurred every 2 sec. in an earphone which she wore.

The *drum room* was a dingy storeroom, partially filled with old apparatus. The walls were dull black. On a table was a battery of six black metal memory drums (5). Only one drum was operated at a time; the duplication of drums saved time in changing lists. The drum advanced every 2 sec. with a dull thud, in marked contrast to the quiet operation of the card-flipper. An additional difference was added by making *S* stand up instead of sit, as he watched the drum. Thus, the two experimental situations differed in incidental visual, auditory, and postural stimulation, but in no other ways.

Design.—All *S*s were given three sessions, which differed only in the interpolated activity.[2] Condition A was a control condition for retention. Original learning, interpolated activity (long division), and relearning were all carried out in the same room under identical incidental stimulation. Condition B was the classical retroactive inhibition experiment, in which the interpolated learning was done in the same room, etc., as was original learning and relearning. In Condition C, our main experimental condition, original learning and relearning were carried out in one room, but the interpolated learning took place in the other room, under contrasting incidental stimulation.

In Experiment I all original learning took place in the drum room. Thirty-six *S*s were

[2] Except that three pairs of word lists were used in counterbalanced order.

scheduled in such a way that the order of conditions A, B, and C, as well as the particular lists used for each, were rotated in a balanced fashion. Experiment II duplicated Experiment I with a different group of *Ss*, except that the rooms were reversed.

Materials.—Each of six lists contained ten pairs of common disyllabic adjectives. Three of the lists were used for original learning, and the other three for interpolated learning. Even-numbered stimulus words on each interpolated list were synonymous with those similarly located on the corresponding original list.[3] The remaining stimulus words and all response words were dissimilar. When the lists were prepared for presentation by drums, the first stimulus word was typed on the first line, and appeared again on the second line with its response word. The third line contained the second stimulus word, etc. The drum advanced every 2 sec., allowing 4 sec. per pair, and 8 sec. (4 blanks) between trials. When the card-flipper was used, the words were printed on cards to yield the same time relations and sequences. The 8-sec. interval between presentations allowed time to return the pack to its starting position.

Subjects.—Seventy-two *Ss* participated in the two experiments, 36 in each, equally divided as to sex. All were students in elementary psychology, or in a non-technical course that follows it. They had had one laboratory session in which they served as both *E* and *S* in a memory experiment with nonsense syllables, employing drums of the same general type used in the present experiment. They were naive with respect to the purpose of the experiment, but highly motivated by the knowledge that they were contributing to an original research problem.

Procedure.—In all cases *S* was asked to try to learn the original list so that he could anticipate the second adjective of each pair before it appeared on the drum. He was given eight trials, which had been found to yield an average of six anticipations—enough to avoid a ceiling of complete learning, while getting sufficiently above zero to show loss through retroaction.

[3] The design of the apparatus made it impractical to vary the sequence of pairs within a given list on successive trials. Hence, there was some serial learning, but it was probably relatively unimportant in comparison with paired association.

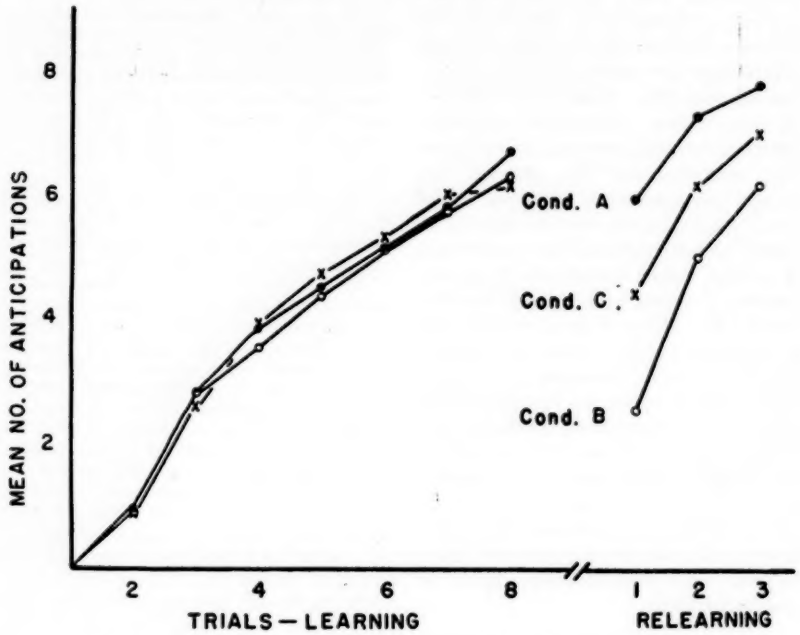

FIG. 1. Acquisition and relearning curves for original lists, based on mean data of the 36 *Ss* of Experiment I

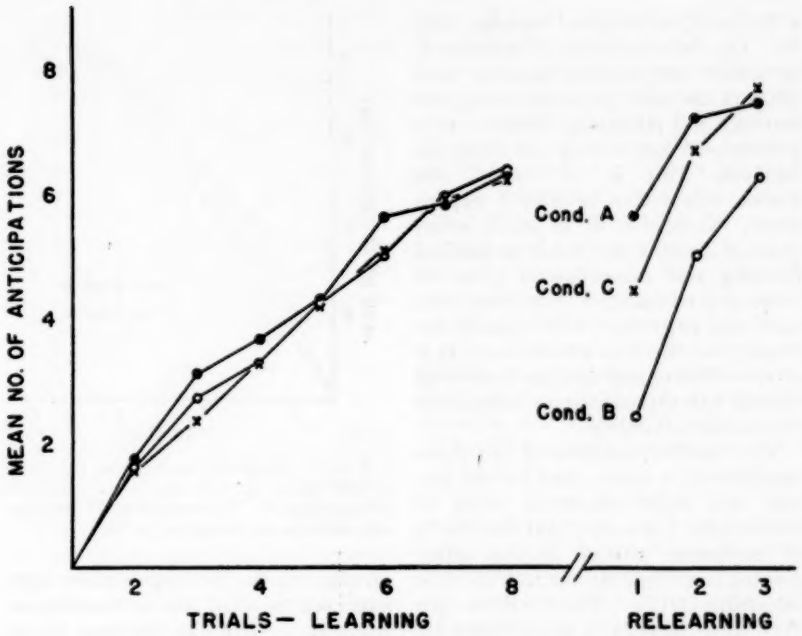

FIG. 2. Acquisition and relearning curves for original lists, based on mean data of the 36 *S*s of Experiment II

He was then given 8 min. of interpolated activity. The first and last 30 sec. of this period were taken up by instructions, changing materials, and escorting *S* to the other room in Condition C. The other 7 min. were spent in long division (Cond. A) or in eight repetitions of the interpolated list in the same room (Cond. B) or in the other room (Cond. C). Immediately following the interpolated activity, *S* relearned the original list to his previous level, or for a minimum of three trials. In Conditions B and C the interpolated list was also relearned to maintain motivation for a later session which might use an interpolated list.

RESULTS

The main results of the two experiments are shown in Figs. 1 and 2, respectively. It will be recalled that the only difference between the two experiments was a reversal of the rooms and other incidental stimulus conditions in the experimental design, to serve as a check on relative effectiveness of the two sets of learning conditions. The two experiments are kept separate, since the striking similarity of the results adds confidence in their accuracy.

In both figures, it is apparent that original learning progressed at the same rate and to the same final level for all three conditions. Furthermore, if the two figures are superimposed, they match very well during this stage of the experiment. It is equally clear that the three conditions differ on the first relearning trial in both experiments. Condition A, in which the interpolated material was long division, shows little, if any, loss during the 8-min. retention interval (although there might be some loss if relearning trial 1 were compared with

a hypothetical original learning trial 9). On the other hand, Condition B, in which interpolated learning took place in the same room as did original learning and relearning, shows a very pronounced loss during the retention interval. This is, of course, the classic retroactive inhibition experiment. Condition C, in which interpolated learning differed from original learning and relearning in terms of incidental stimulation from room, posture, and apparatus, falls midway between the other two conditions. It is obvious that changing these incidental stimuli has approximately halved the retroactive inhibition.

The relative positions of the three conditions are maintained for the second and third relearning trials of Experiment I, showing that the effects of incidental stimuli during interpolated learning continue well into the relearning trials. These effects are even more marked in Experiment II, for Condition C approaches Condition A on the second relearning trial, and surpasses it on the third trial. The differences between the two conditions on trials 2 and 3 are not significant. To put it another way, in Experiment II there was no evidence after the first relearning trial that learning an interpolated list in a strange room had more retroactive inhibiting effect than did interpolated long division. This is largely the result of more rapid relearning in Condition C of Experiment II. There is no obvious reason why this happened; it is possible that it is a chance event, for no statistical comparisons were made between Experiments I and II.

One major question remains before the results are taken at their face value. Was the interpolated learning equal in amount under Conditions B and C? If *S* had learned less when the interpolated list was given in the strange room, he might have been expected to show less retroactive inhibition. When the learning curves were plotted for lists interpolated under the two conditions, as in Fig. 3, the curve for Condition C, Experiment I, fell consistently about a half-syllable below that for Condition B, but the differences between it and its companion condition were not statistically significant. Conditions B and C, Experiment II, would fall between these two curves, if they were plotted in Fig. 3. Hence, we can dismiss the possibility that the observed differences found between Conditions B and C during relearning were due to differences in efficiency of learning the interpolated lists.

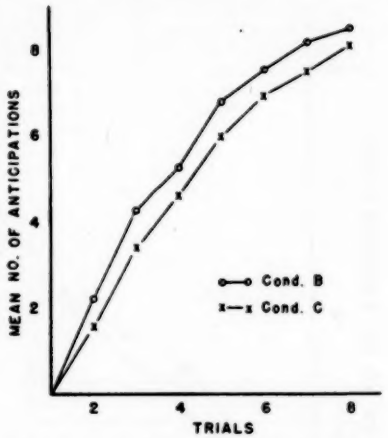

FIG. 3. Acquisition curves for the interpolated lists in the two experimental conditions of Experiment I. The mean number of anticipations on each trial represents 36 *Ss*.

The chief results of the experiment were so clear that they could be demonstrated convincingly through graphic presentation of the data. However, all of the statements made above have been verified by analysis of variance. It seems unnecessary to reproduce these extensive tables here.

since they are available elsewhere.[4] The analyses brought out the fact that the women were consistently and significantly superior to the men in learning ability. The difference was of the order of magnitude of 10 per cent, and showed in all stages of the experiment. Since men and women were equally represented in all groups, it seemed legitimate to pool their results for presentation. Separate analyses for each sex showed that they reacted in the same way to the main experimental variables.

Discussion

It has long been known that retroactive inhibition is a function of the similarity of the materials used in the original and interpolated learning. The present experiment extends the generalization to similarity of the conditions under which the materials are learned. It was shown that the amount of retroactive inhibition can be approximately halved by presenting the interpolated material in a different room, with different apparatus, and with different posture. The results are understandable in terms of an interference theory of retroactive inhibition if we assume that the incidental stimuli resulting from the changed situations serve to support a discrimination between original and (similar) interpolated lists; i.e., they serve as discriminative stimuli.

The results of this experiment are in marked contrast with those of Nagge (4), who failed to find a decrease in retroactive inhibition when interpolated learning took place in a room different from that used during original learning and relearning. There are many factors that could account for the differences in results. In the first place, incidental stimulation may well be a relatively minor part of the total stimulation, since *S* attends to the content of the learning material rather than to the method of presentation. It is possible that differences in this incidental stimulation are of importance only when original and interpolated lists are highly similar in content; these are the only differential stimuli available to support a discrimination between the two lists. It should be noted that in the present experiment half of the interpolated stimulus words were good synonyms of correspondingly located words in the original list. Under lesser degrees of similarity of content, there might well be little or no decrease in retroactive inhibition as a result of changing incidental stimulation.

A second difference between the present experiment and that of Nagge would seem to be the *amount* of change in incidental stimulation. Nagge did not describe the differences between his two rooms, but it is unlikely that they were as great as those in the present experiment; a cheerful classroom *vs.* a dingy darkroom. In addition, we varied the exposure apparatus and *S*'s posture. The magnitude of these changes may well account for our positive results. It should be noted, however, that neither of our experimental situations showed a relatively unfavorable influence on rate of learning. Further, there were no changes in the basic sense modality, the effector organ employed, or the general condition of *S*; only incidental stimuli and responses were involved in the changes. Apparently, these incidental changes furnished enough differential stimuli to aid the discrimination between synonyms in original and interpolated lists, and, thus, to dimin-

[4] The Bilodeau (1) thesis contains appendices which include the raw data, as well as detailed descriptions of the experimental design and conditions.

ish interference between the corresponding response words.

It would be interesting to assess the relative importance of room, exposure device, and posture in furnishing incidental discriminative stimulation. Unfortunately this cannot be done from the present experiment. In view of Nagge's negative results, the experiment was set up to produce the largest differences possible, with enough *Ss* to give statistical significance to the results. If breakdown of the effects of the whole pattern of changes is desired, it will be necessary to set up an experiment designed for the purpose.

Summary and Conclusions

1. These experiments were designed to test the hypothesis that retroactive inhibition would be decreased if the similarity between the environments in which original and interpolated learning took place was reduced by conducting the interpolation in a different room, with different apparatus, and with different posture. The original learning consisted of eight repetitions of ten paired-associate adjectives, which were relearned in the original ("standard") learning situation, following 8 min. of interpolated activity. In the first experiment each of 36 undergraduates relearned an original list after each of the following interpolated conditions:

A. Eight min. of long division, in the original room.

B. Eight presentations of a second list of ten paired-associate adjectives, in which five of the stimulus words were synonomous with five in the original list, under the standard conditions (standing and watching a memory drum in a darkroom).

C. Interpolation of the second list under contrasting experimental conditions (reading the list from cards while sitting in a classroom).

In the second experiment the same schedule was repeated with an additional 36 *Ss*, but the situations designated "standard" and "contrasting" were reversed.

2. The results of both experiments showed that the interpolated list lost approximately half its retroactive effect on recall if learned under conditions markedly different from those of original learning. In the first experiment the reduction in retroactive inhibition remained at about one-half throughout the three relearning trials, but the effect of a change in situation was even more striking in the second experiment, where Conditions A and C were indistinguishable after the second relearning trial.

3. There were no statistically significant differences between the conditions in rate of learning, on either the original or interpolated lists.

4. These results are consistent with an interference theory of retroactive inhibition. However, they do not indicate the *relative* importance of stimuli arising from the room, the apparatus, or the *S*'s posture, respectively, in aiding the discrimination between the original and interpolated learning.

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THE EFFECT OF AN EXTRA STIMULUS UPON STRENGTH OF RESPONSE DURING ACQUISITION AND EXTINCTION [1]

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A stimulus extraneous to the instigation of any given response may have varied effects upon that response. The extraneous stimulus may interfere with the response and in greatly varying degrees; if so, we commonly speak of "distraction" or "disturbance." Pavlov termed such an effect "external inhibition" (**17**). On the other hand, an extraneous stimulus has also been reported to facilitate a response. We can think of no everyday terms for such an effect, but certain psychologists (**3, 13, 14**) have called it "dynamogenesis." When Pavlov found such an effect of an extraneous stimulus on a response undergoing extinction, he considered that it had a selective and destructive effect on the accumulating inhibition, and called the effect "disinhibition" (**17**).

The conditions which control the direction and the degree of the effect of an extraneous stimulus are undoubtedly complex. So far, however, this problem has received little systematic attention. The present investigation concerns the direction and degree of change in the strength of a response that is associated with the introduction of an extraneous stimulus at various stages of the acquisition and extinction of that response. This study was suggested partly by results from a similar study reported by Gagné and partly by consideration of the dynamogenic effects of extraneous stimuli which have been reported (**5, 10, 13**).

Gagné's study (**6**) concerned the effects of a buzzer and a scratch, applied to the back of the starting box, on strength of the running response of a white rat in the straight elevated maze devised by Graham and Gagné (**8**). He measured the strength of this response in terms of the animal's latency of starting to run. His results showed that an extraneous stimulus may have opposite effects upon the strength of such a response when it is introduced during acquisition and extinction. Neither buzzer nor scratch had an appreciable effect upon the latent period when they were applied just before the first trial of acquisition. When introduced later, however, the buzzer was associated with a depression in strength of response (increased latency) at both the fourth trial of acquisition and the fifth trial of extinction, but on the fifth extinction trial was followed by an increment in strength on the next trial. The scratch was associated with: (*a*) significant depression of response on the fourth trial of acquisition, and (*b*) significant facilitation of response on the fifth trial of extinction. Gagné explained these results by assuming for the buzzer an emotional effect which he agreed with Skinner (**19**) would act regularly to increase the amount of "internal" inhibition and thereby always produce an immediate depression of response. As the inhibition died away, he argued that ". . . spontaneous recovery may be thought of as occurring to produce an increased magnitude of response on the subsequent trial" (**6**, p. 114). He used the opposite effects of the scratch during acquisition and extinction as an argument for the Pavlovian concept of inhibition, with the response aroused by

[1] From the Psychological Laboratory of Brown University. This experiment was reported at the 1946 meeting of the Eastern Psychological Association at Fordham University. This paper derives from the M.Sc. thesis by the first author.

the scratch interfering with the dominant excitatory process during acquisition, and with a dominant inhibitory process during extinction.

In considering these results, certain clinical observations and the reports that extraneous stimuli employed in distraction experiments sometimes facilitate rather than interfere with complex activities (**5, 10, 13**) led us to consider that something like so-called dynamogenic effects found there might possibly be demonstrable with rats on the elevated maze. If demonstrable, it would seem that an extraneous stimulus could be associated with opposite directions of change in the strength of a response even during acquisition. Furthermore, we questioned the generalization made by both Gagné (**6**) and Skinner (**19**) that the effect of an emotional stimulus is generally inhibitory or depressing. In counseling, e.g., one hears the same individual report times when the effects of stage fright were devastating for the performance and other times when the stage fright seems to the individual to facilitate the excellence of a performance. Moreover, one common way of helping a person to deal with stage fright is to have him overlearn the performance concerned.

Similarly, we considered it possible that introduction of an extraneous stimulus might have opposite effects on the magnitude of a response at different stages of extinction. Finally, we considered that if opposite effects at different stages of both acquisition and extinction were demonstrable on such a simple response system as running down an elevated runway, this fact would have considerable theoretical significance.

The present investigation is a repetition and extension of Gagné's study, using the buzzer as the extraneous stimulus and introducing it at several stages during the acquisition and extinction of the simple running response employed by Gagné. The purpose of this study is to test the hypothesis that quite different effects upon response strength may be secured from the same extra stimulus, depending upon the point during acquisition or extinction at which it is introduced.

Method

Apparatus.—The apparatus employed in the present study was identical with that used in all the runway studies at Brown University and has been described in the original study by Graham and Gagné (**8**). It consisted of a runway 3 ft. long and 1½ in. wide. At either end of the runway was a small platform 6 in. by 8 in., on which were placed the experimental boxes, which served as starting boxes and food boxes on alternate trials. These boxes were carried by means of a handle at the back. In front of the two platforms were screens out of which square portions had been cut in order to make the runway accessible to the starting or food boxes. Doors at either end of the runway could be raised or lowered by means of a pulley system. These served to open or close the starting or food boxes. An electric buzzer was mounted on one of the supports about 10 in. below the platform at the starting end of the runway.

The entire apparatus was inclosed by a framework measuring 2 ft. by 4 ft. The runway was illuminated from above by a 25-w. bulb which was suspended from the center of the top of the framework. The framework was covered on top and on the sides with cheesecloth, which provided a one-way screen and served to eliminate visual cues from outside of the experimental situation.

Subjects.—The *S*s employed in this experiment were 50 albino rats of a Wistar strain, of which 31 were males and 19 were females. In addition, seven rats were discarded when they failed to leave the starting box after 5 min., and three died of a respiratory disease during the training period. The age of the *S*s was between three and four months. All of the *S*s had been reared under standard colony conditions, and none had been used in previous experiments.

Procedure.—The *S*s were "tamed" for a period of two weeks, during which time their living cages were kept in the experimental room, and they were fed on a table approximately the same time each day for 30 min. The purpose of this procedure was to permit them to become habituated to handling by *E* and to the experimental room.

Following the two weeks of habituation, for 10 min. a day on four consecutive days the *S*s were "trained" without incentive by allowing them to explore an elevated runway of the same color and width as the experimental runway. On these four days, following their exploration, *S*s were fed for 20 min. in one of the experimental boxes. On that day preceding its serving in the experiment, *S* was fed for only 12 min. The food used during this pre-experimental habituation was Purina mash.

The experiment was conducted 24 hr. after the 12-min. feeding following the final trial of exploration on the elevated runway. An experimental session consisted of 15 equally spaced trials of acquisition and 8 equally spaced extinction trials. At the beginning of an experimental session, *S* was taken from the living cage and placed in the starting box with the door closed.

A pellet of food of approximately 0.5 gm. was placed in the food box, and the door opening into the food box was raised. A stop watch was turned on when the door to the starting box was raised at the beginning of a trial, and was stopped when *S*'s body, exclusive of the tail, passed a point 4 in. from the starting box. This interval, the *latent period* of the response to the raising of the door, is our measure of response strength.

Once *S* entered the food box, the door behind it was closed, and it was permitted to eat the pellet of food. The next trial began 75 sec. after *S* arrived in the food box. Sixty-five sec. after its arrival, the box was carried to the starting position, and 10 sec. later the starting door was raised for the next trial. Animals were carried in the experimental box, rather than by hand, to avoid the emotional effects of handling reported by Gagné and Graham (7).

The 15 acquisition trials were followed, after the regular 75-sec. interval, by the first extinction trial. The procedure during extinction differed from that during acquisition only by the fact that the food was omitted on these trials.

The extraneous stimulus, a fairly loud buzzer, was sounded for 4 sec. and discontinued 2 sec. before the door to the starting box was opened to start the "buzzer trials."

Five groups of ten *S*s were used in five different experimental procedures. Group I was a control group in which the 15 acquisition and 8 extinction trials were measured in the usual way and without the presentation of the extraneous buzzer. Group II received the buzzer before the fourth trial of acquisition and the second trial of extinction. Group III received the buzzer before the eighth trial of acquisition and the fourth of extinction. In Group IV the buzzer was sounded before the twelfth acquisition and the fifth extinction trials. In Group V the buzzer was sounded before the fourteenth trial of acquisition and the sixth trial of extinction.

Results

The data from this experiment are the time intervals, in seconds, required for the rats to respond to the opening of the door of the starting box with movement down the runway to a point 4 in. beyond the door of the starting box. This interval is the *latent period*, and it is a measure of response strength. In order to render these intervals proper for statistical treatment, they have been converted into logarithmic scores, which yield a more nearly normal distribution.

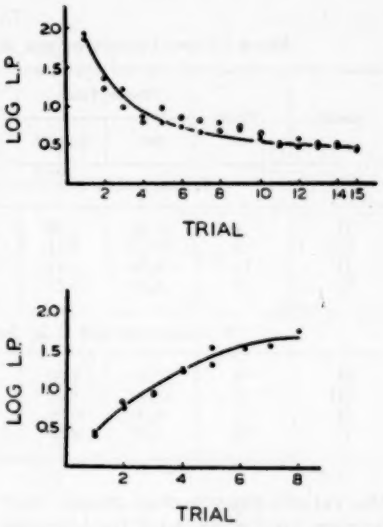

Fig. 1. The course of acquisition (top) and of extinction (bottom) of the running response in the control group (I). Solid circles represent the means of the logarithms of the latent periods for this group. Data obtained by Graham and Gagné (8, p. 260) are shown by the unfilled circles. The best-fitting lines are drawn through the points.

Acquisition and extinction for the control group.—The means of the log latencies of the trials of acquisition and extinction for the ten rats in the control group are represented graphically in Fig. 1. The solid lines (best fitting) through the points represent the general course of acquisition (above) and extinction (below). Comparison of these results with those of Graham and Gagné (8, p. 260) reveals that substantially the same curves were obtained.

Effect of the buzzer during acquisition.—The effect of the buzzer on the latency of the response may be demonstrated by either of two comparisons. The latencies of the buzzer trials for

TABLE I

MEAN LATENT PERIODS OF THE BUZZER TRIALS AND NO-BUZZER TRIALS

Group	Trial	Buzzer Trials		No-Buzzer Trials		Difference		
		Sec.	Log LP	Sec.	Log LP	Sec.	Log LP	*P*
A. Comparison with Corresponding Trial of Control Group								
II	4	31.40	1.50	7.41	0.87	23.99	0.63	.01
III	8	17.40	1.24	6.31	0.80	11.09	0.44	.01
IV	12	6.90	0.84	3.72	0.57	3.18	0.27	>.05
V	14	3.89	0.59	3.10	0.49	0.79	0.10	>.05
B. Comparison with Trials Immediately Preceding Buzzer Trials								
II	4	31.40	1.50	9.12	0.96	22.28	0.54	.01
III	8	17.40	1.24	8.13	0.91	9.27	0.33	.01
IV	12	6.90	0.84	4.27	0.63	2.63	0.21	.05
V	14	3.89	0.59	2.30	0.36	1.59	0.23	.01

the various experimental groups may be compared either with the latencies of the corresponding trials for the control group, or with the latencies of each experimental group's own preceding trial. Both comparisons yield the same general picture (see Table I and Fig. 2) except for presentation of the buzzer at the fourteenth trial. However, since some of the experimental groups were "faster" and others "slower" than the control group, the latency on the trial preceding the buzzer trial for each group is probably the more relevant for assessing the effect of the buzzer at successive stages of acquisition. It may be seen from Fig. 2 that the differences obtained in this manner are the smaller of the two, except in the case of Group V on trial 14, where the control group showed a longer mean log latent period than did Group V on trial 13.

Three elements appear in the general picture of the average effect obtained at the successive stages of acquisition. First, the average effect is always interference (lengthened latent period) with the response of running to the stimulus of the opening of the starting box door. Individual exceptions to this average effect occurred when the buzzer was sounded at the eighth, twelfth, and fourteenth trials. At the eighth acquisition trial, one, and at the twelfth and fourteenth each, three of the animals actually ran more quickly on the buzzer trial than they had on the preceding trial. Whether these are genuine instances of facilitation (dynamogenesis) or are merely the result of random oscillations in response strength cannot be determined from this experiment.

Secondly, as may be observed from either Table I or Fig. 2, the absolute differences between the latent periods of the buzzer trials and either of their controls tend to diminish at successive stages of acquisition. Thus, the absolute amount of interference would appear to diminish as the running response becomes stronger. This trend is general for comparisons in terms of both seconds and log latent period, with the values for corresponding trials of the control group (Table I, *A*). It is also true for comparisons between latent periods in seconds for the buzzer trials and the preceding

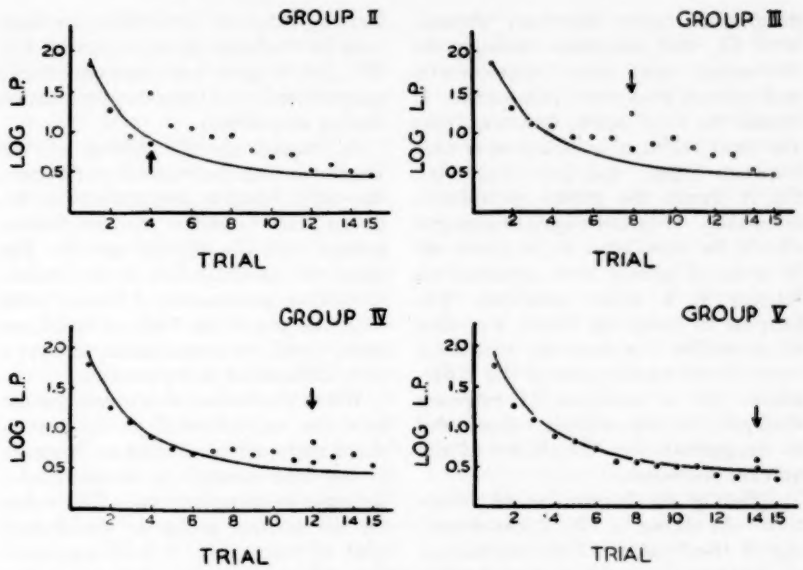

FIG. 2. Effect of the buzzer when sounded before the fourth, eighth, twelfth, or fourteenth acquisition trial in Groups II through V. The arrow indicates the trial before which the buzzer was sounded, and the solid point is the corresponding value for the control group (Group I) on that trial. The curve drawn on each graph is the acquisition curve for the control group.

trials of the same group (Table I, *B*). However, an exception appears for comparison of the log latent period of the buzzer trial for Group V and that of their trial 13. Moreover, on trial 14 for Group V the effect of the buzzer ceases to be statistically significant.

Thirdly, the relative amount of interference also appears to diminish as the running response increases in strength, at least until the fourteenth trial of acquisition. When the differences in the means of the latent periods are treated as percentages of the means of the values of the controls, as is done in Table II, one sees that by all methods of comparison the per cent decrease in response latency resulting

TABLE II

PERCENTAGE CHANGE IN LATENCY OF RESPONSE AS A FUNCTION OF SOUNDING A BUZZER BEFORE THE TRIAL

Group	Buzzer Trial	Difference between Experimental and Control *Ss* on Comparable Trials		Difference between Experimental *Ss* on Buzzer Trial and Preceding Trial	
		Sec.	Log LP	Sec.	Log LP
II	4	323%	76%	244%	56%
III	8	174	55	114	36
IV	12	88	49	62	33
V	14	25	20	69	62

from the buzzer decreases through trial 12, and decreases through the fourteenth trial when experimental and control groups are compared. It should be said again, however, that the most relevant comparison is that between buzzer and preceding trial, for it keeps the group of animals common. Whether response strength should be considered to be measured in terms of latency or in terms of log latency is a moot question. The purpose in using log values was only to normalize the data for statistical tests of the significance of the differences. As a measure of response strength, the logarithmic values tend to exaggerate the variability of the quicker responses.

Effect of the buzzer during extinction.—As shown in Fig. 3, the sounding of the buzzer before the second, fourth, fifth, and sixth trials of extinction for the animals in Groups II, III, IV, and V gave less clear-cut results than those found from its presentation during acquisition.

In examining the effects of the buzzer during the trials of extinction, the only feasible comparison is between the latencies of the buzzer groups and the control group. Because of the sharp rise in the extinction curve, comparison of buzzer trials with the preceding trials in the same group would be meaningless, or at any rate, difficult of interpretation.

When the buzzer was presented before the second trial of extinction, there apparently resulted an increase in response strength as revealed by a decrease in mean latency. The value for the control group at the second trial of extinction is 0.79 log units

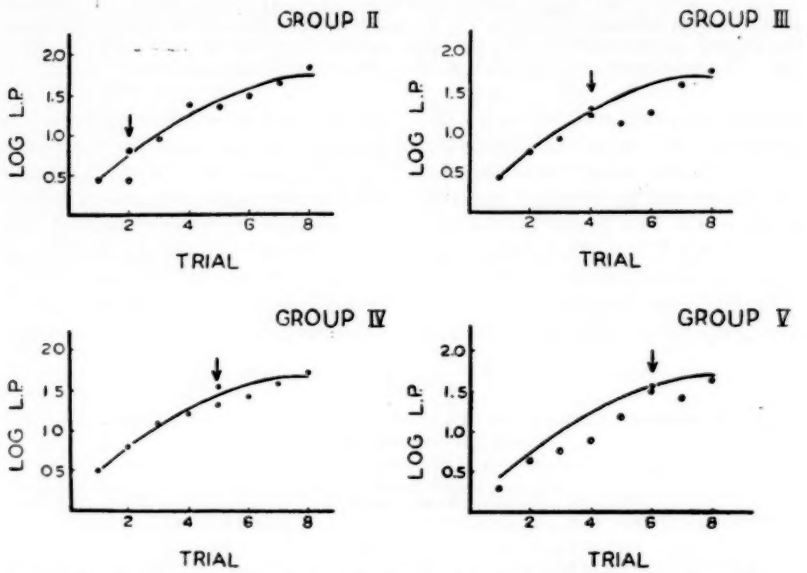

FIG. 3. Effect of the buzzer when sounded before the second, fourth, fifth, or sixth extinction trial in Groups II through V. The arrow indicates the trial before which the buzzer was sounded, and the solid point is the corresponding value for the control (Group I) on that trial. The curve drawn on each graph is the extinction curve for the control group.

(6.2 sec.), while the corresponding value for Group II is 0.45 (2.8 sec.). This difference of 0.34 log units was found by the *t*-test to be significant at the .05 level of confidence. From these results it appears that sounding the buzzer has prevented the decrement in strength of response which usually occurs following the frustrating effects of the first extinction trial.

The value of the log latency at the fourth trial for the rats of Group III is almost identical (0.06 less) with the corresponding value for the control group. In other words, the buzzer does not seem to have appreciably altered the decrement in response strength which is to be expected at this stage of extinction. However, a decrease in mean latency appeared on the three subsequent trials for this group. On the fifth trial, the value for the control group is 1.56 log units (36.1 sec.), and for Group III is 1.13 (17.9 sec.). This difference (0.43) was found by the *t*-test to be significant at the .05 level. The differences between Group III and the control group at the sixth and seventh trials were not statistically significant.

Sounding the buzzer before the fifth trial of extinction is again associated with a decrement in latency indicating an increase in response strength. The difference of 0.21 log units between the mean latencies of Group IV and controls, however, was found not to be significant even at the .05 level of confidence. Some indication of a decrease in mean log latency is also present on the sixth and seventh trials for Group IV, but these were not significant.

These results obtained from Group IV may be contrasted with those of Gagné (6, p. 111), who found that a buzzer sounded before the fifth trial of extinction resulted in an increased latency on that trial, followed by a decrease in latency on the following trial. These contradictory findings are difficult to explain; they may, however, be attributed to the difference in the stage of acquisition at which the buzzer was sounded. During acquisition the *Ss* used by Gagné received the buzzer before the first or fourth trial. In the present experiment, the *Ss* of Group IV received the buzzer before the twelfth acquisition trial. It is possible that the difference in the effect of the buzzer sounded during extinction bears some relation to the effect it has had during acquisition, or to the proximity of the previous presentation. Such a relationship, however, is probably complicated by the amount of extinction which has taken place when the buzzer is sounded for the second time.

Comparing the mean log latency for Group V, in which the *Ss* received the buzzer before the sixth extinction trial, with that of the control group on the sixth trial shows that the buzzer had little effect. The difference is only 0.03 log units. In fact, none of the differences between the mean log latencies of Group V and the control group on the sixth, seventh, or eighth trials is statistically significant.

On the other hand, it is evident from Fig. 3 that the extinction data for Group V fit the curve for the control group poorly. By inspection, the discrepancy appears larger than that for any of the other experimental groups, and for every trial the mean latency for Group V lies below that for the control group. Although the animals in Group V may have differed from the control *Ss* this consistent discrepancy during the extinction trials might be attributable to the fact that the buzzer was sounded at the fourteenth trial of acquisition in Group V, only two trials away from the point at which the extinction trials

began. Sounding the buzzer so close to the beginning of extinction trials may have tended to increase the resistance of this group of animals to extinction. If this be true, inspection of the trend of latency values (Fig. 3) for Group V gives a hint that sounding the buzzer on the sixth trial of extinction may possibly have had an effect opposite in direction to that found for the second extinction trial, namely, to increase latency or decrease strength of response. A similar view can also be taken for the results shown by Group III.

Discussion

This experiment has shown clearly that the magnitude of the weakening effect of an extraneous stimulus is a function of the stage of acquisition at which it is introduced. Statistically significant increments in mean log latency were found when the buzzer was sounded before the fourth, eighth, and twelfth trials of acquisition. Furthermore, the magnitude of these increments in mean latency decreased with the number of the acquisition trial until, at the fourteenth, it was small and not statistically significant. These findings agree with Pavlov's prediction (**17**, p. 45), not systematically verified by experimentation so far as we know, that a given external inhibitor will have less effect during the later stages of learning than during the early ones. These results also confirm Lindberg's statement (**15**, p. 470) that the action of an external agent depends among other things on the stability and strength of the established conditioned response. The findings also corroborate those distraction studies which have found that the interference of a distractor decreases as a performance becomes more "automatic" (**1**, **2**, **4**, **20**, **22**).

The effect of the buzzer on the mean latency of response at these four stages of acquisition offers no confirmation of the possibility, suggested by the reported dynamogenic effects of distractors, that an extraneous stimulus may facilitate stable and well-learned responses. However, six animals exhibited decreased latencies when the buzzer was sounded late in learning (three at the twelfth trial and three at the fourteenth). Although these cases may be based upon random variation (oscillation) in response strength, they hint that a change in the direction of effect of the buzzer even during the course of acquisition may be possible. An experiment employing more trials of acquisition may be worth while to provide a more adequate test of this possibility.

On the whole, the results derived from introducing the extraneous stimulus during extinction are less clear than those for acquisition. Yet sounding the buzzer at the second extinction trial (Group II) was definitely associated with an increment of response strength. Although there may be dubious justification for speaking in Pavlovian terms of disinhibition after only one trial, the effect is like disinhibition. The fact that the latency of the second extinction trial equals, but is not less than, that of the first, agrees with the findings of others (**18**, **21**, **24**) that the strength of a response resulting from disinhibition is never greater than that on the last trial of acquisition.

Because of the proximity of the buzzer-sounding to the last reinforced trial in Group II, the increased response strength found might also be considered in Pavlovian terms as an inhibition of the inhibition which has been built up during reinforcement. This has been termed by Hovland (**11**) as "inhibition of reinforcement." However, the fact that this is the only group of animals of the five in which the latency of the second extinction trial is not greater than that of the first indicates that nonreinforcement ordi-

narily has a depressing effect on the response of running the elevated runway.

The other results during extinction, unclear as they are from one standpoint, afford difficulty for Pavlovian theory. In so far as the comparisons between experimental and control groups are concerned, when the buzzer was sounded before the fourth, fifth, and sixth extinction trials, it appears to have been associated with small increments in response strength, and also possibly with increments in strength for one or two subsequent trials. Statistically these differences are not significant, and one can quite as well consider the obtained results from another point of view. If one takes as the basis of reference the line joining the latency values for the trials immediately preceding and succeeding that on which the buzzer was sounded, the effect of the buzzer may be seen for both Groups III and V as a decrement in response strength during these later extinction trials. This would mean that the direction of effect did change when the extraneous stimulus was introduced at successive stages of extinction. Even though the results are equivocal on this latter point, nothing in Pavlovian theory can account for the variation in the effect of the buzzer during extinction. Furthermore, we see difficulty with Gagné's (6) interpretation that during acquisition the extraneous stimulus interferes selectively with excitation because excitation is dominant, and that during extinction it interferes selectively with inhibition because inhibition is dominant. In this experiment, the extraneous stimulus interfered considerably more with what is considered inhibition in Pavlovian terms on the second trial of extinction, when the inhibitory process is presumably at its lowest level of dominance, than on any later trial.

One can explain most of the obtained results in terms of interfering reaction systems. First, we may assume that the buzzer would tend to produce a neuromuscular response of its own, and, indeed, the animals were often observed to huddle or crouch as the buzzer was sounded. Secondly, since the buzzer is of constant strength and is sounded but twice for each animal, once during acquisition and once during extinction, we can justifiably assume that this response to the buzzer is of relatively constant average strength from one group of rats to another. Thirdly, we know from the data that the running response is increasing in strength with successive reinforcements during acquisition. From these propositions we may infer: (*a*) that the effect of the buzzer should always depress the strength of the running response during acquisition, and (*b*) that the amount of depressive effect should diminish during successive stages of acquisition, possibly to a degree where it would no longer be observable. This is essentially the view of Guthrie (9) and Wendt (23), and it accounts nicely for the progressively diminishing increments of mean latency observed at the successive stages of acquisition. Such an interpretation would also appear to derive from Hull's (12) theory. Since there is no room in such a formulation to explain the facilitative effects of an extraneous stimulus, those observed in individual animals would have to be considered to result from random oscillations of response strength.

Although it is more difficult, with some added assumptions this sort of formulation can also be made to account for the results obtained during extinction. The first additional assumption would be that the conditions of nonreinforcement set up a response system (a "not-to-run" response) that competes with the response being extinguished.[2] Such an assumption has commonly been made by Guthrie (9), Hull (12), Miller and Dollard (16), and Wendt (23). Thus, the extraneous stimulus must be considered during extinction to act upon the dynamic interrelations between the two competing response systems.

The ordinary facilitative effect ob-

[2] It is not important here to consider the nature of these conditions that reinforce the competing response system. They may be fatigue derived from running the alley (16) or, in this situation, possibly an emotional response anthropomorphically analogous to disappointment.

served for extraneous stimuli during extinction might be accounted for by assuming that the response aroused by these stimuli interferes selectively with the newer or possibly the less stable of the two competing responses. The fact that the greatest facilitative effect of the buzzer occurred after the first nonreinforced trial in this experiment suggests that *newer* would be the more appropriate adjective here. On the other hand, the variety of the effects observed during extinction by Gagné (6), Skinner (19), and ourselves suggests that the interrelations involved here are complicated, but perhaps very important for further study. One might also make this formulation explain the dynamogenic effects that have been reported by assuming that they occurred in subjects who were in some degree of conflict. The important thing to discover in this connection is the law or laws which govern the selectivity of the effect of the extraneous stimulus.

If it should ever be demonstrated that the depressing effect of an extraneous stimulus does change to a facilitative effect with overlearning of simple responses, such a formulation as we have used here would no longer be tenable. Such a result would require the envisagement of a kind of mechanism, probably of a central neural nature, whereby the excitement aroused by an extraneous stimulus could interfere with a response at one stage or degree of organization and facilitate it at a later stage or greater degree of its organization. Unless such an empirical result should be clearly demonstrated, however, the more traditional kind of theory outlined can serve well.

Summary

1. This investigation, stimulated partly through a similar study by Gagné and partly from consideration of the dynamogenic effects reported for distractors, has been concerned with the direction and degree of change in the magnitude of a response that is associated with the introduction of an extraneous stimulus at various stages of the acquisition and extinction of that response.

2. White rats ran an elevated maze, and the measure of strength of response was the time taken by the rat to leave the starting box after the door had been lifted. The extraneous stimulus was a buzzer which was sounded for about 4 sec. and ceased about 2 sec. before the door of the starting box was lifted to start the trial.

3. Five groups of ten *S*s were given 15 trials of acquisition and 8 trials of extinction. Group I served as controls. In Groups II through V the buzzer was sounded before the fourth acquisition and second extinction trials, before the eighth acquisition and fourth extinction trials, before the twelfth acquisition and fifth extinction trials, or before the fourteenth acquisition and sixth extinction trials, respectively.

4. The average effect of introducing the buzzer during acquisition is a consistent decrement in strength (increased latency) of the response. The decrement decreases as the running response becomes stronger with successive reinforcements. There were individual exceptions to these averaged results.

5. When the buzzer was introduced before the second extinction trial, the mean latency of response was reduced to what it had been on the first extinction trial. When the buzzer was introduced before the fourth, fifth, or sixth extinction trial, the effect was small and equivocal in direction.

6. The results are explained by means of an interference theory, but it is pointed out that such a theory would be inadequate if further investigation should demonstrate that an extraneous stimulus can produce effects of opposite direction in the course of both acquisition and extinction.

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RESISTANCE TO EXTINCTION AND THE PATTERN OF REINFORCEMENT: II. EFFECT OF SUCCESSIVE ALTERNATION OF BLOCKS OF REINFORCED AND UNREINFORCED TRIALS UPON THE CONDITIONED EYELID RESPONSE TO LIGHT[1]

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The present experiment along with three previous studies (**4, 5, 7**) was concerned with the identification of a class of stimulus variables related to resistance to extinction. It was concerned specifically with the effect upon resistance to extinction of the number of times during training that reinforced trials followed unreinforced trials. Human *S*s were presented with training sequences consisting of varying numbers of successive, random alternations of blocks of reinforced trials and blocks of unreinforced trials. All *S*s received the same number of reinforced trials, but it was thought that those *S*s for whom there were numerous transitions from unreinforced trials back to reinforced trials might be especially resistant to extinction, because they had an opportunity to learn to respond following unreinforced or extinction trials.

Besides the number of transitions from unreinforced to reinforced trials the average size of the blocks of unreinforced trials was varied. It was possible to combine this second variable with the number of transitions in a three-by-three orthogonal design so that the two main effects and their interaction might be evaluated.

[1] This experiment was supported in part by the Research Committee of the Graduate School of the University of Wisconsin from funds provided by the State Legislature.

[2] Now at Johns Hopkins University.

Apparatus and Procedure

Apparatus.—The apparatus and general procedure have been described elsewhere (**6, 7**). The conditioned stimulus (CS) consisted of an increase in brightness of a 10-cm. circular milk glass window from 14 to 345 millilamberts. The stimulus window was 60 cm. directly in front of the *S* and was fixated binocularly. The unconditioned stimulus (UCS) was a puff of air delivered to the right cornea. The CS lasted 0.75 sec., and the UCS, when given, followed the onset of the CS by 0.45 sec. A 30-cm. square, white diffusing surface surrounded the stimulus window. This surface was maintained at a brightness which varied spatially from 14 to 35 millilamberts in order to prevent dark adaptation of the *S*.

Experimental design.—The design of the experiment is shown in Table I, which outlines the Day 1 procedure for each of the nine experimental groups. In Table I, it will be noted that all three groups in any one column receive the same number of transitions from unreinforced to reinforced trials. Similarly, all three groups in any one row receive the same average number of trials in each block of unreinforced trials. The number of transitions is thus orthogonal to the average number of trials per unreinforced block.

Procedures.—The experiment required an hour session on each of two successive days for all procedures. On Day 1 the *S* received 1, 3, or 5 transitions from unreinforced to reinforced trials, with an average of 2, 4, or 6 trials per unreinforced block, depending upon the group to which he had been assigned. He received a total of 30 reinforced trials on Day 1. On the second experimental day the procedure was the same as the first, except that the average number of unreinforced trials per block was cut in half to 1, 2, and 3, respectively, and only 15 reinforced trials were given. On both days the acquisition sequences began and ended with reinforced trials. After the last reinforced trials of Day 2, an extinction series of 20 unreinforced trials was given.

Because it was not feasible to hold constant

TABLE I

DAY 1 PROCEDURES FOR THE NINE EXPERIMENTAL GROUPS

Group A 1 transition 2 unreinforced trials per block	*Group D* 3 transitions 2 unreinforced trials per block *	*Group G* 5 transitions 2 unreinforced trials per block
Group B 1 transition 4 unreinforced trials per block	*Group E* 3 transitions 4 unreinforced trials per block	*Group H* 5 transitions 4 unreinforced trials per block
Group C 1 transition 6 unreinforced trials per block	*Group F* 3 transitions 6 unreinforced trials per block	*Group I* 5 transitions 6 unreinforced trials per block

* The number of unreinforced trials per block is an average figure. The actual number was varied slightly from block to block so as to avoid simple cues due to regularity. The blocks of unreinforced trials were spaced at approximately even intervals throughout the training series. For example, the blocks of unreinforced trials in Groups D, E, and F, center column above, were placed after the 9th, 15th, and 22nd reinforced trials on Day 1, and after the 6th, 8th, and 11th reinforced trials on Day 2.

both the total number of reinforced trials and the total number of trials, the total number of trials varies from procedure to procedure, increasing progressively from the upper left corner to the lower right corner of Table I. This variable, along with per cent reinforcement, remains confounded with the main effects and the interaction, but within the range of the present experiment, there is evidence that this effect is minimal.

Subjects.—The *S*s were 90 men and women students in elementary psychology classes. Subjects were assigned in order of their appearance to the nine groups until there were ten in each.

RESULTS

All eyelid closures with latencies from 0.15 to 0.75 sec. during extinction were considered to be CR's. Analysis of the latency distribution indicated that sensitized β-responses to light were almost totally absent (6). Since the experiment was concerned primarily with the behavior of the CR's during extinction, it is important to remark that the average extinction trends for all groups were essentially regular and showed none of the systematic reversals reported by Humphreys (3) for random reinforcement, or by Grant, Riopelle, and Hake (7) for systematic alternation of reinforcement. The mean frequency and variance of CR's during extinction are

TABLE II

MEANS AND VARIANCES OF FREQUENCY OF RESPONSE OF THE GROUPS DURING FINAL EXTINCTION (N = 10 EACH GROUP)

Group A		*Group D*		*Group G*	
Mean:	3.5	Mean:	7.2	Mean:	6.0
Variance:	18.9	Variance:	65.9	Variance:	40.2
Group B		*Group E*		*Group H*	
Mean:	5.0	Mean:	12.3	Mean:	10.0
Variance:	34.0	Variance:	49.0	Variance:	45.0
Group C		*Group F*		*Group I*	
Mean:	7.4	Mean:	8.3	Mean:	7.8
Variance:	47.2	Variance:	45.0	Variance:	26.96

TABLE III

SUMMARY OF ANALYSIS OF VARIANCE OF FREQUENCY OF RESPONSE DURING FINAL EXTINCTION

Source of Variation	*ss*	*df*	*ms*	*F*
Number of alternations	244.47	2	122.24	2.66
Average unreinforced block size	192.27	2	96.14	2.09
Interaction	109.46	4	27.62	
Replication	3722.30	81	45.95	

reported for each group in Table II. The effects of number of alternations of reinforcement and average block size are shown graphically in Fig. 1, where the total number of CR's during extinction is plotted as a function of the number of alternations of reinforcement in the left-hand graph and as a function of average block size in the right-hand graph. Frequency of response appears to increase to a maximum and then to decrease as a function of the number of alternations and also as a function of average block size.

There is a slight positive correlation between the means and variances of Table II, but this is slight enough to be disregarded in the statistical analysis (1, 3). The analysis of variance of the data of Table II is summarized in Table III. In spite of the 81 degrees of freedom available for the error estimate, the *F*'s of 2.66 and 2.09 do not achieve significance at the 5 per cent level. The *F* for interaction is less than 1.00. Thus neither of the two main effects—number of transitions or average block size—nor their interaction is statistically significant.

The maxima of Fig. 1 may be the reason for failure of the trends of Table II to reach statistical significance. For example, the rise in frequency from the one transition to the three transition groups in the left-hand figure is significant at the 5 per cent level ($t = 2.31$ with 28 *df*), although the rise from the two-trial to

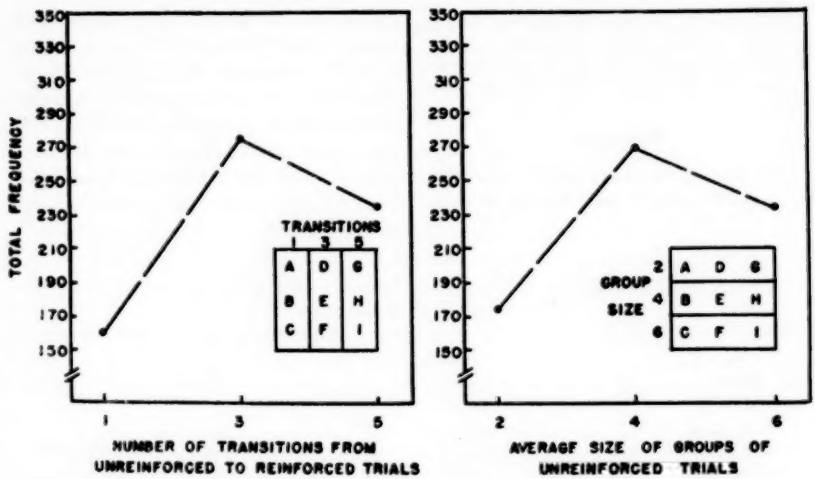

FIG. 1. Frequency of response during the final 20 extinction trials is plotted as a function of number of alternations of reinforcement (left) and average unreinforced block size (right). The legend of the abscissa on the right indicates block sizes for Day 1 only. The groups received average unreinforced block sizes of 1, 2, or 3 trials on Day 2.

the four-trial groups in the right-hand figure is not ($t = 1.89$ with 28 df). To claim that there is an optimal number of transitions from unreinforced to reinforced trials which will maximize resistance to extinction would be premature if based on the present findings. That there is such a maximum is probable, for this view is supported by other evidence from the Wisconsin Laboratory.[3]

An analysis of the average magnitude of the CR's during extinction has also been completed, but it is not presented here because of its essential similarity to the analysis of the frequency data.

Discussion

The results of the present experiment add to, rather than decrease, the recognized complexities of partial reinforcement phenomena (**8, 10, 11,** *et al.*). They point up the fact that these phenomena are not only incompletely understood, but, also, that there is much which remains unknown at a simple descriptive level. Certainly the notion that resistance to extinction increases simply with an increased number of transitions back to reinforced from unreinforced trials must be dismissed as inadequate. The results suggest either that the number of transitions does not affect resistance to extinction or that resistance to extinction shows one or more maxima for optimal numbers of transitions. The writers favor the latter interpretation.

A precise relation between the present results and those of Humphreys (**9**) is hard to establish. As the number of transitions increases and the average number of trials per unreinforced block decreases, the stimulus sequence approaches Humphreys' random reinforcement. The complexity of the situation then becomes apparent when the considerable resistance to extinction of Humphreys' *S*s is viewed in the light of the present possibility of maximum resistance to extinction occurring after as few as six alternations of reinforced and unreinforced trials. It is possible that a very large number of transitions changes the whole situation markedly so that the present findings are not necessarily opposed to those of Humphreys.

One complicating factor in the partial reinforcement experiment is the relatively slow acquisition of the eyelid CR. The *S* is acquiring the eyelid CR; he is learning about the sequence of stimuli; these processes may or may not be interacting; and, finally, his responses are adjusting to the discriminated sequence of stimuli. It is well known that *S* recognizes trial patterns and sequences at a verbal level, but that his eyelid responses do not follow his verbal discriminations (**2, 7**). To eliminate the problem of acquisition of a new response, a further experiment (*cf.* footnote 3) was carried out, and the *S*'s adjustment to partial reinforcement will be discussed in a second paper based on the similar, but statistically more adequate, findings of that experiment.

Summary

Resistance to extinction of the eyelid CR's of 90 human *S*s was studied as a function of the number of transitions from unreinforced back to reinforced trials, and the average length per block of the unreinforced trials. The total frequency of CR's during extinction increased with the number

[3] A second study utilizing Humphreys' conditioned verbal response has been completed in which a similar maximum was found. This independent observation was thought to justify testing for the maximum obtained in the present study.

of transitions to a maximum and then decreased. A similar, although less reliable, trend resulted for average unreinforced block size.

It was pointed out that the results emphasize the descriptive complexity of partial reinforcement phenomena and preclude premature theorizing. Further work has been carried out on a different response system and is reported in a second paper.

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RESISTANCE TO EXTINCTION AND THE PATTERN OF REINFORCEMENT: III. THE EFFECT OF TRIAL PATTERNING IN VERBAL "CONDITIONING" [1]

BY HAROLD W. HAKE,[2] DAVID A. GRANT, AND JOHN P. HORNSETH [2]

University of Wisconsin

This experiment was carried out to discover the effects of (*a*) the number of transitions from blocks of "unreinforced" trials back to "reinforced" trials and (*b*) the length of the block of unreinforced trials upon performance during extinction of the Humphreys' conditioned verbal response (**10**). The investigation was part of a program (**4**, **5**, **6**, **7**) to discover factors in the sequences of trials which affect resistance to extinction in the partial reinforcement situation.

In a previous experiment on the conditioned eyelid response it was discovered that the maximum number of CR's occurred during extinction if there had been three transitions from unreinforced to reinforced trials during each day's acquisition sequence (**7**). Three transitions were significantly (5 per cent level) more effective than one and possibly more effective than five transitions per day in producing resistance to extinction.

Because the eyelid results were presumably complicated by the slow acquisition of the eyelid CR, and because of ambiguity in the statistical tests, it was desirable to repeat the experiment with a more readily acquired response. Humphreys' technique was chosen because it depends upon highly developed verbal behavior and because it closely parallels some aspects of conditioning situations.

[1] This research was supported by the Research Committee of the Graduate School of the University of Wisconsin from funds provided by the State Legislature.

[2] Now at Johns Hopkins University.

Apparatus and Procedure

Apparatus.—The apparatus consisted of a flat black plane surface 3 ft. square which was set vertically on a desk in front of a small classroom. On either side of the board 1 ft. down from the top a 60-w. frosted light bulb was mounted so that the *S*s, seated in the classroom, looked at the top of the bulbs. An electronic timing circuit provided for a 3-sec. illumination of the left-hand bulb, which could be followed at its termination by a brief flash of the right-hand bulb.

Experimental design.—The design, the same as that of the previous study, is shown in Table I. The two variables, number of transitions from unreinforced to reinforced trials and average number of trials per unreinforced block, each occur at three levels and are orthogonal so that a total of nine procedures make up the three-row by three-column table. In the table the assignment of groups to columns was made in terms of the number of transitions from unreinforced to reinforced trials, and the assignment of groups to rows was in terms of the average number of trials in the unreinforced blocks. The design permits independent evaluation of the main effects of the two factors, transitions and block size, and of their interaction.

Procedures.—At the beginning of each trial, the left-hand bulb was illuminated for 3 sec. During *positive* or reinforced trials the right-hand bulb was illuminated for ½ sec. at the end of the 3-sec. flash. During *negative* or unreinforced trials the right-hand bulb remained unlighted. Trials were separated by about 30 sec.

The *S*s were told at the beginning of the experiment that they were participating in a study of guessing ability and that whenever the left-hand light appeared, they were required to guess whether the right-hand light would also flash. All *S*s in a particular group were run at the same time, and responses were recorded on IBM answer sheets by each *S*. During the experiment the *S*s were carefully proctored to see that they recorded their responses after the left light and before the right light flashed.

One experimental session of ½-hr. duration was required for each group of *S*s. During the training procedure each group received 30 positive trials randomly interspersed with blocks of

TABLE I

DAY 1 PROCEDURES FOR THE NINE EXPERIMENTAL GROUPS

Group A 1 transition 2 unreinforced trials per block	*Group D* 3 transitions 2 unreinforced trials per block *	*Group G* 5 transitions 2 unreinforced trials per block
Group B 1 transition 4 unreinforced trials per block	*Group E* 3 transitions 4 unreinforced trials per block	*Group H* 5 transitions 4 unreinforced trials per block
Group C 1 transition 6 unreinforced trials per block	*Group F* 3 transitions 6 unreinforced trials per block	*Group I* 5 transitions 6 unreinforced trials per block

* The number of unreinforced trials per block is an average figure. The actual number was varied slightly from block to block so as to avoid simple cues due to regularity. The blocks of unreinforced trials were spaced at approximately even intervals throughout the training series. For example, the blocks of unreinforced trials in Groups D, E, and F, center column above, were placed after the 9th, 15th, and 22nd reinforced trials on Day 1.

negative trials, as indicated in Table I. After the last training trial an extinction[3] series of 20 negative trials was given. The procedure, then, closely parallels the procedures used in the eyelid response experiment except that only one session was needed for each *S*.

Subjects.—The *S*s were 180 volunteer men and women students in elementary classes in psychology. Twenty *S*s were assigned to each of the nine experimental groups according to the time of appearance of each group.

RESULTS

The *S* could make two kinds of responses, a *positive response*—that the second light would flash on—or a *negative response*—that the second light would not flash on. Resistance to extinction was measured by the number of positive responses emitted by each *S* during the 20-trial extinction series. Since, as was the case with the eyelid conditioning study, the extinction curves of the nine groups were monotonic decreasing functions of the extinction trial number, and no reversals of slope, such as those observed by Humphreys (**9**, **10**), were found, the extinction scores serve as general indices of persistence of responses rather than indicators of extinction trend reversals.

[3] Extinction is used in a purely descriptive sense in that the frequency of a response is reduced under conditions analogous to the extinction of a CR.

TABLE II

MEANS AND VARIANCES OF FREQUENCY OF RESPONSE OF THE GROUPS DURING FINAL EXTINCTION (N = 20 EACH GROUP)

Group A		*Group D*		*Group G*	
Mean:	5.15	Mean:	5.85	Mean:	4.95
Variance:	14.92	Variance:	12.13	Variance:	6.04
Group B		*Group E*		*Group H*	
Mean:	2.65	Mean:	4.85	Mean:	4.65
Variance:	1.83	Variance:	8.53	Variance:	3.72
Group C		*Group F*		*Group I*	
Mean:	2.15	Mean:	4.35	Mean:	4.65
Variance:	0.53	Variance:	5.83	Variance:	4.63

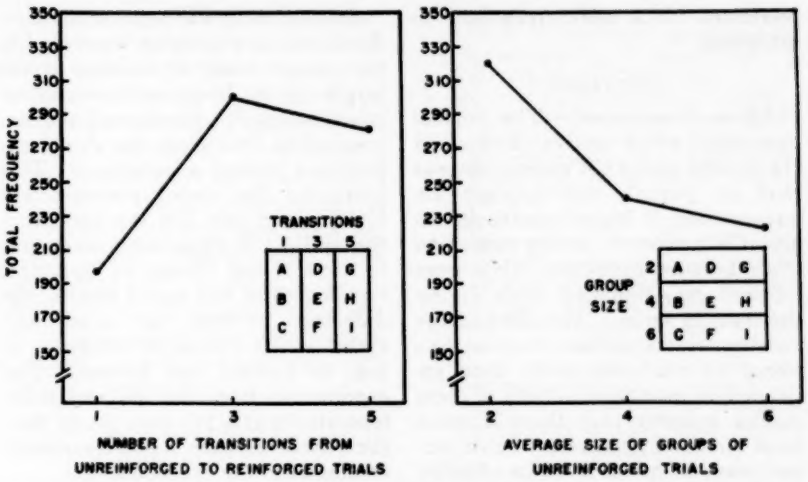

FIG. 1. Frequency of positive responses during the final 20 extinction trials, plotted as a function of number of alternations of reinforcement (left) and average unreinforced block size (right)

In Table II are presented the mean and variance[4] of the frequency of positive responses in each of the nine groups of the experiment. The groups are in the same spatial arrangement in Table II as they were in Table I. In Fig. 1 the frequency of positive responses during extinction is plotted on the left-hand side as a function of the number of transitions from negative to positive trials, and on the right-hand side as a function of the average size of groups of negative or unreinforced trials. The left-hand graph shows a maximum for three alternations as did the corresponding eyelid CR data. The right-hand graph shows that frequency of positive responses during extinction was a monotonic decreasing function of the number of negative trials per block during the training series. This latter curve does not show the same trend as the eyelid CR counterpart.

The data of Table II were analyzed statistically, and the analysis of variance summary is given in Table III. The main effects are highly significant ($P < .001$), and the interaction fails to reach significance. Thus, it may be concluded that both the number of transitions from unreinforced to reinforced trials during training and the number of negative trials in the un-

TABLE III

SUMMARY OF ANALYSIS OF VARIANCE OF FREQUENCY OF "YES" RESPONSES DURING FINAL EXTINCTION

Source of Variation	*ss*	*df*	*ms*	*F*
Number of alternations	100.31	2	50.16	7.39
Average unreinforced block size	85.51	2	42.76	6.30
Interaction	42.36	4	10.59	1.56
Replication	1161.35	171	6.79	

[4] Heterogeneity of variance, and a slight correlation between means and variance occurs in Table II. It is felt, however, that the high degree of significance of the *F*'s in the subsequent analysis of variance justifies considerable confidence in the statistical significance of the findings (1, 2).

reinforced block affect resistance to extinction.

Discussion

Effect of transitions.—The original hypothesis which motivated this and the parallel eyelid CR experiment was that in partial reinforcement sequences the *S* learns specifically to give CR's after or during extinction trials because extinction trials were followed by reinforced trials during the training series. The effect of the number of transitions from unreinforced to reinforced trials upon resistance to extinction in both of these studies suggests that there is some basis for this hypothesis. Other factors seem to be involved in addition to the one originally hypothesized, however, because resistance to extinction appears either to be a negatively accelerated function of the number of transitions or to have a maximum in the neighborhood of three transitions per day in the range tested. Two factors might easily account for the negative acceleration. First, as the number of transitions increased, the number of negative trials increased from group to group in these experiments. Thus, a simple extinction process during training might ultimately reduce the favorable effect of transitions on resistance to extinction as the number of transitions increased. Secondly, as the transitions increase beyond an optimal frequency, the *S* may learn to discriminate the transitions, as such, more readily and may learn to change his response quickly to fit the discriminated changes in stimulus sequences. This could be a more general learning set (**8**), or it might occur, in part, simply because there are more transitions from reinforced to unreinforced trials as well as more transitions from unreinforced to reinforced trials.

Effect of size of unreinforced blocks.—Resistance to extinction was found in the present study to decrease as the length of the blocks of unreinforced trials increased. In contrast, the corresponding data from the eyelid experiment showed a maximum. This trend for the verbal response was highly significant, but the data from the eyelid CR experiment were not. In view of the absence of statistical significance of the eyelid results, the difference between the verbal and eyelid results will not be stressed. It may be pointed out, however, that experiments from this (**3**) and other laboratories (**10, 11**) have shown that the verbal response is not completely analogous to the eyelid CR.

The downward trend of resistance to extinction as a function of the number of trials in the negative or unreinforced blocks can be explained by two processes. First, the more negative trials during training the more a general extinctive effect might be expected which should lower resistance to subsequent extinction. Secondly, the longer blocks of negative trials "stand out" more in the training series and hence may be more readily discriminated. The *S* can thus learn more easily to give negative responses to negative trials—another learning set phenomenon perhaps—and thus will be quicker to start giving negative responses during extinction. Of these two possibilities the second seems more promising, although no definitive test has yet been made.

Summary

The effects of (*a*) the number of transitions from unreinforced to reinforced trials, and (*b*) the average number of trials in the unreinforced blocks on the extinction of Humphreys' verbal response were studied in 180 *S*s by means of a three-by-three orthogo-

nal experiment. The function relating resistance to extinction to the number of transitions had a maximum at three transitions, which had previously been observed in a parallel eyelid CR experiment. Resistance to extinction was a monotonic decreasing function of the average number of unreinforced or negative trials per block. These findings were interpreted tentatively in terms of the *S*'s discriminating these specific features of the sequence of training trials and reacting to them as well as to the isolated stimuli.

(Manuscript received March 20, 1950)

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COMBINED DRIVES IN LEARNING

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Various studies (**3, 4, 6**) support to some degree the assumption that irrelevant drives motivate habits which have been set up on the basis of different drive conditions. In these studies it was assumed that there were no opposing habits evoked by the "extra" drives when the organism was present in the test environment.

Problem

With equal strengths of two opposed habits acquired under different single-drive states, how would various drive combinations affect the contradictory response strengths?

This investigation studied the combined drives of hunger and thirst when, for each of the single drives, opposing responses had received an equal number of trials on the same T-maze.

The following specific hypotheses were tested with rats first trained under single alternating drives to choose one side for water and the other for food:

Hypothesis I.—Given approximately equal habit strengths acquired under alternating single drives, there will be interference of responses when the drives are combined.

Hypothesis II.—As the "drive elements" in the combined drives approach equality (as measured by hours of deprivation), this interference will increase, and chance performance will occur when the drives are equal in hours of deprivation.

These hypotheses were based on the assumptions that: (1) the habit strengths of the opposing response patterns were approximately equal, (2) the drive strengths could be measured by hours of deprivation, and (3) the drives were not changed in strength when put in combinations. The assumption that drive interaction does not greatly change a drive state is supported by Kendler's (**4**) combined-drive study with rats rewarded differentially for hunger and thirst. Their performance indicated later that they had acquired differential responses for the single drives. Physiological interaction may, however, prevent a simple summation of drive strength as well as interfere, to some degree, with evocation of responses previously learned under single drives.

Hypothesis III.—The interference of the opposing habits will delay the appearance of responses appropriate to only one drive in the trials under combined drives. It is expected that animals with established contradictory habits cannot learn as rapidly to respond appropriately to only one drive *when both are present*, as can control animals who have not previously acquired opposing response patterns under single drives.

Procedure

Animals.—Twenty-eight male, Wistar-strain, naive rats, approximately 90 days old, were used in Experiment I. In Experiment II, nine male rats of the same strain and age were used, but they had received previous training under single alternating drives and under the same condi-

tions as those of the present study. They were retrained at the start of Experiment II.

Apparatus.—A simple T-maze was used in a fixed position in a small room with walls painted black. A complete description of the room, feeding schedule, and other details may be found in Eschenbach (1) or Stone (5). An illuminated circular white disc remained in the same fixed position at the end of the right arm of the T-maze as a place cue during both experiments.

Experiment I.—Following preliminary training on a straight runway, the 28 *Ss* were divided into four groups which were equated for right- and left-turning tendencies as determined by previous tests. All trials, except the trial after 30-hr. deprivation, were given at the rate of one per day with the non-correction method, and time scores in seconds were recorded from the starting position to the reward platform. The trials after 30-hr. deprivation required a two-day interval between trials in order to obtain a 30-hr. deprivation and still run the *Ss* at the same time during the day.

In *Group 1* (Experimental) ten *Ss* were trained under alternating single drives of 23½-hr. deprivation to choose the right side of the T for food and the left side for water. In order to discover if an alternation habit was being formed, the pattern of alternation was occasionally broken by having two or three food, or water, days in succession. The learning was not significantly disturbed, thus indicating that an alternation habit was not formed. The criterion of learning was the following: All *Ss* in the group, i.e., the entire group as a group, performed correctly for ten successive choices under single drives. Sixty-eight days were required to reach this criterion of learning. Each *S* was then given three trials (days) under each of the following combined drives in the following order: 22-hr. *water* deprivation with 4, 8, 12, 16, 22, and 30 hr. of *food* deprivation. They were rewarded with water only on the left side of the maze on each of the three test days under combined drives. Relearning under single drives occurred after each three-day combined-drive test. The criterion of relearning was two successive correct trials (days). The longest period of such relearning was six days.

In *Group 2* (Experimental) seven *Ss* were run by the same procedure as used in Group 1 except that under single drives they found water on the right and food on the left. They were rewarded only with water on the right side in the combined-drive tests.

In *Group 3* (Control) six *Ss* of the same strain and age were trained not under single drives, but under the same combined drives as the experimental *Ss*, at the same time, and by the same procedure. They found water reward (the only one given) on the left side of the T-maze. The control *Ss* were run only when the experimental *Ss* were ready for their tests under combined drives. On the days when control *Ss* were not run, they remained in their cages and were on the same feeding schedule as the experimental *Ss*. They received no training at any time under single drives.

In *Group 4* (Control) the five *Ss* differed only from Group 3 in that they obtained the water reward on the right side.

Experiment II.—With nine *Ss* the procedure used was that outlined above for the experimental *Ss* except that the combined drives were 22-hr. food deprivation combined with each of the following hours of water deprivation: 4, 8, 12, 16, 22, and 30, and the only reward was food. Two groups, five in one, and four in the other, differed only in the place of food reward and water reward. These nine *Ss* reached the criterion of learning described in Experiment I in 37 trials. These *Ss* had previously received similar training under single drives. Again, relearning with single drives occurred to a criterion of two correct trials (days) between combined-drive performances. The longest time for relearning under single drives was six days. No control *Ss* were used in this experiment.

Results

For the experimental *Ss*, Fig. 1 shows the percentage of interference from the varying drive as it is added in increasing amounts to a constant 22-hr. deprivation. Results for both Experiments I and II indicate close similarity, although the drive combinations were reversed. For the control *Ss* no opposing responses were associated at any time with the single drives. And, as shown in Fig. 1, for the control *Ss* the introduction of the increasing amount of the extra drive was not correlated with interference in responding correctly to the rewarded 22-hr. deprivation. Hypothesis I was substantiated: With approximately equal habit strengths acquired under single drives, interference of the opposing responses increased as the extra drive was added to the 22-hr. deprivation.

However, the chance performance predicted for the experimental *Ss* by

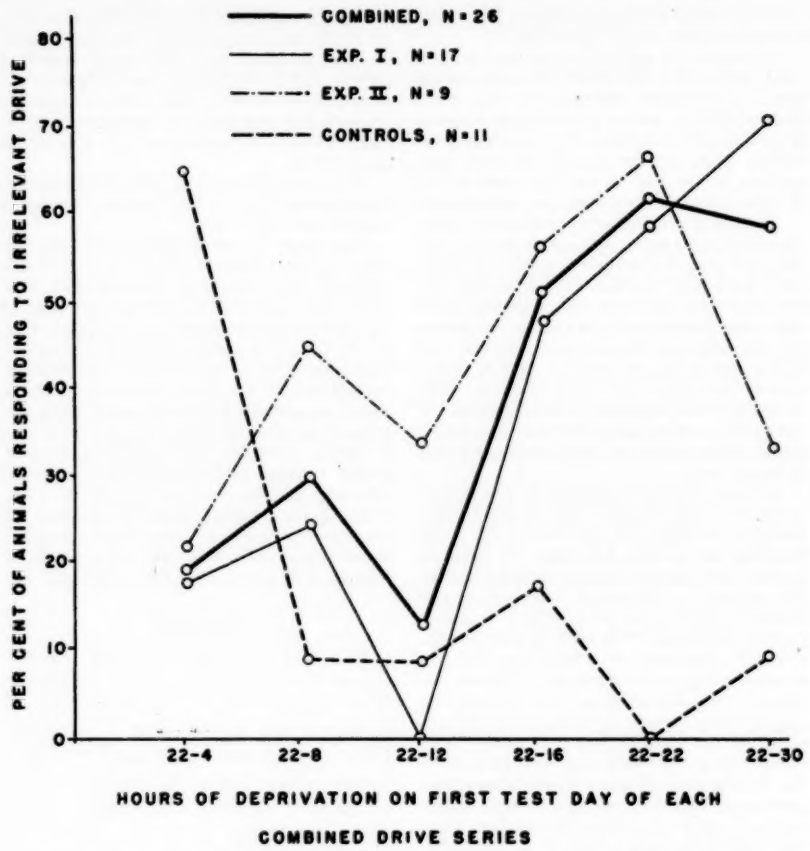

FIG. 1. Percentage of *Ss* in experimental and control groups responding to an irrelevant drive on the first day of each combined-drive series (Experiments I and II)

Hypothesis II for equal combinations of deprivations—22-hr. thirst and 22-hr. hunger—actually occurs, as shown in Fig. 1, at 22-16. Assuming a simple interaction of D_1 and D_2, in which D_1 is drive for 22-hr. deprivation and D_2 is the variable drive, we would not expect the dip in the line at 22-12, nor the marked rise at 22-16, followed by a deceleration. These variations in the line are not statistically significant, but it may be noted that the same trend appears in both experiments. Experiment I used a constant 22-hr. food deprivation and Experiment II a 22-hr. water deprivation. The same amounts of extra drive (4, 8, 12, 16, 22, 30) were added and in the same order. Other orders and amounts of extra drive are to be tested in further experimentation.

Figure 1, it should be emphasized,

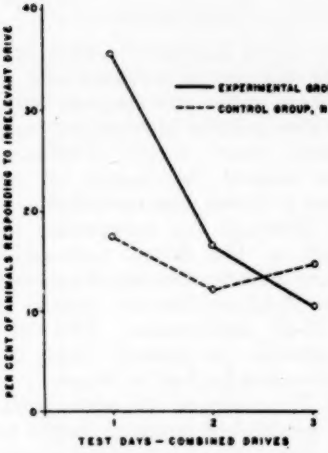

FIG. 2. Percentage of *Ss* in Experiment I who responded to the irrelevant (unrewarded) drive on the first, second, and third days of combined-drive series

is an expression of the degree to which the *Ss* responded on the *first* trial to the constant 22-hr. deprivation whatever the drive combinations being tested. Each test of combined drives consisted, however, of three rewarded runs to the drive which had suffered constant 22-hr. deprivation. It was possible that the reward during the combined-drive test days had an increasing effect on the experimental *Ss*, although between tests the opposing habits for single drives were reestablished.

By Hypothesis III, it was expected that the experimental *Ss*, with established contradictory habits, could not learn as rapidly to respond appropriately to only one drive when both were present, as could control *Ss* who had not previously acquired opposing response patterns under single drives.

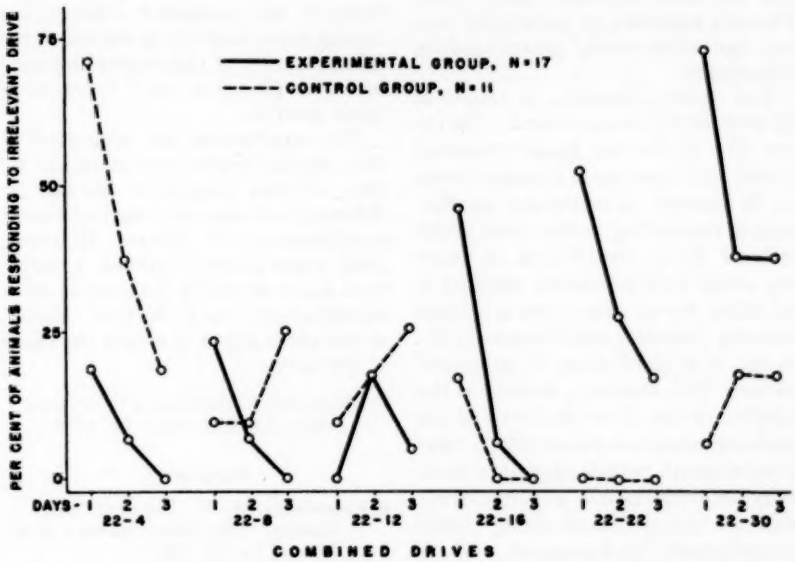

FIG. 3. Percentage of *Ss* in the experimental and control groups responding to the irrelevant (unrewarded) drive for each combined-drive series (Experiment I)

Figure 2 shows the improvement that occurred in the apparent learning during the combined-drive test series for both control and experimental *Ss*. Figure 3 again indicates this rapid adjustment of the experimental *Ss*, particularly on the second and third of the three combined test days.

The rapid adjustment of the experimental *Ss* to respond appropriately to the rewarded side in combined-drive series was, of course, aided by experimental extinction of the nonrewarded (irrelevant) drive. But it should be emphasized that these experimental *Ss* had received frequent reinforcement under single drives both in the original training and retraining. And as Hilgard points out, one or two extinction trials should not so easily counteract frequent reinforcements "if it is merely sensori-motor responses which are controlled by reinforcement and non-reinforcement" (**2**, p. 338). Theories emphasizing perceptual control can more easily accept sudden adjustment.

The results, therefore, in regard to Hypothesis III are equivocal. On the *first* day of the test under combined drives, the responses of the experimental *Ss* showed an increasing interference in responding to the drive which suffered 22-hr. deprivation as there was added to it increasing amounts of the other drive. But there is a rapid recovery from this interference on the second and third days of combined drives. This recovery, defined as the isolation of the 22-hr. element and the resulting correct response only to that drive element, would suggest a flexibility in performance not consistent with a theory emphasizing habit strength under reinforcement. There is, however, no evidence within this experiment of the reason for this rapid recovery from interference.

Conclusions

Our three hypotheses were supported with certain modifications:

I. Rats with approximately equal habit strengths for opposed responses acquired under single alternating drives showed interference of responses as drives were combined.

II. Although this interference increased as the drives approached equality, chance performance occurred at the 16-22 combination instead of the 22-22 combination. This was true whether the constant 22-hr. deprivation was for food or water.

III. Experimental *Ss* with previously established opposing habits to single drives were not as rapid as control *Ss* in the development of responses to only one element of the two-drive combinations. However, the experimental *Ss* quickly adjusted their performance on the second and third test days of the combined drives, thus raising some question in regard to the interpretation of the original learning under single drives as a function of habit growth.

The conclusions are supported in that similar results were obtained by two different groups of *Ss* run by different experimenters and in different combinations of drives. However, both experiments involved a single fixed order of testing the various drive combinations, and further experiments are required to isolate the effect of this factor.

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